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A paper to be presented at the 142nd Meeting of the American Institute of Electrical Engineers, New York, April 25th, 1900.

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HYSTERESIS IN SHEET IRON AND STEEL.

BY ARTHUR HILLYER FORD.

Since the general introduction of stations for the supply of light and power, by the use of alternating currents, the subject of hysteresis loss in iron has been an important study from the standpoint of the efficiency of such installations. In the direct current system the only place where hysteresis loss occurs is in the armatures of the dynamos and motors; while the alternating current system has in addition the loss in transformer cores, which is apt to be larger than all other electrical losses combined, in the case of a station operating 24 hours per day. Therefore a slight increase in this loss may make a decided difference in the efficiency. Take as an example a two K.W. transformer having a copper loss of 56 watts and an iron loss of 42 watts which gives a maximum efficiency of 95.2% and an all-day efficiency of 86.7% on a basis of full load for four hours and the remainder of the time no load. If the iron loss is increased 20% the efficiencies become 95.0% and 84.8%

During the latter part of 1894, attention was called to the fact that there is an increase in the core loss (hysteresis and eddy current) of a transformer, or that the iron aged with use, in a letter by G. W. Partridge[1] to the *London Electrician*. This called forth a communication from J. A. Fleming,[2] citing similar observations which had been made in 1892. This aging was first

1. "Increase of Open Circuit Loss in Transformers with Time." *Electrician* (London) Vol. 34, p. 161.

2. "Time Increase of the Open Circuit Loss in Transformers." *Ibid*, Vol. 34, p. 190.

thought to be due to magnetic fatigue, but is has been shown by O. T. Blathy,[3] W. M. Mordey,[4] J. A. Ewing,[5] and others to be purely a heat effect.

The method used by Ewing was to subject the iron to rapid reversals of magnetism, at the same time keeping it cool, when no increase of the hysteresis loss was noticed. That used by Mordey was to make two cores from the same sample of iron and heat one to a given temperature by magnetic reversals and the other to the same temperature from some external source. On measuring the hysteresis loss it was found to increase at the same rate in both cores, thus showing that this increase is a pure heat effect. This method has been used by others, who took iron cores and found no aging when they were subjected to an alternating magnetic field and kept cool, but on allowing them to get hot, there was an immediate increase in the hysteresis loss.

Up to the present time there have been no tests published which throw any light on the cause of the increase of this loss.

This research was undertaken for the purpose of discovering if possible what the cause of this effect is, in order to find how it may be reduced. That it may be reduced seems probable to everyone who has studied the subject, for there are some irons which show this effect very slightly. Experiments have been carried out in a similar direction by F. Guilbert[6] which tend to show that chemical composition has no appreciable effect on the hysteresis loss in soft sheet iron.

This research is divided into two parts. I.—That on commercial transformers, which was undertaken with the object of showing the magnitude of the aging under conditions met in practice. II.—That on small specimens of iron obtained from various makers and users.

Thanks are extended to the various companies who furnished the iron and transformers for this series of tests and to those who made the chemical analyses.

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3. *Electrician* (London), Vol. 34, p. 191.

4. "On Slow Changes in the Permeability of Iron." *Ibid*, Vol. 34, p. 219.

5. "Is the Magnetic Quality of Iron Affected by often Repeated Reversals?" *Ibid* Vol. 34, p. 297.

6. "On the Law of Hysteresis." *L'Eclairage Electrique*, Vol 6, pp. 357, 390.

I.—Measurement of the Core Losses of Eight Transformers to Show the Magnitude of the Aging Effect.

These transformers were all obtained between June 1894 and October 1896 and had been used none, or for a few hours only, for testing purposes in the laboratory. They were connected with the high pressure coil of one, feeding the high pressure coil of the next, whose low pressure coil fed the low pressure coil of

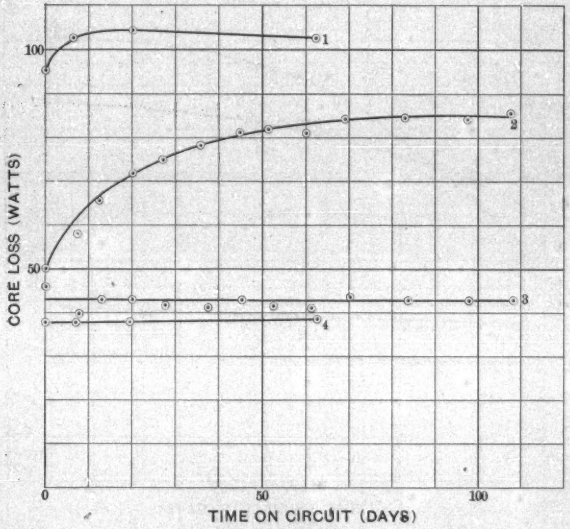

Fig. 1.

the third, and so on, with a lamp load on the low pressure coil of the last transformers. By requiring the large transformers to supply lamps, in addition to the power for the smaller ones, the load on each was made about two-thirds of its rated capacity and was kept on continuously with the exception of a few hours each week while the station was shut down.

The transformer capacities and the increase in the core losses are shown in Table I, and Figs. 1 and 2.

TABLE I.

No.	Capacity.	Increase in Core Loss.
1	1.5 K.W	25%
2	1.5 "	95%
3	1.5 "	2%
4	1.5 "	4%
5	1.25 "	113%
6	2.5 "	38%
7	1.5 "	20%
8	1. "	10%

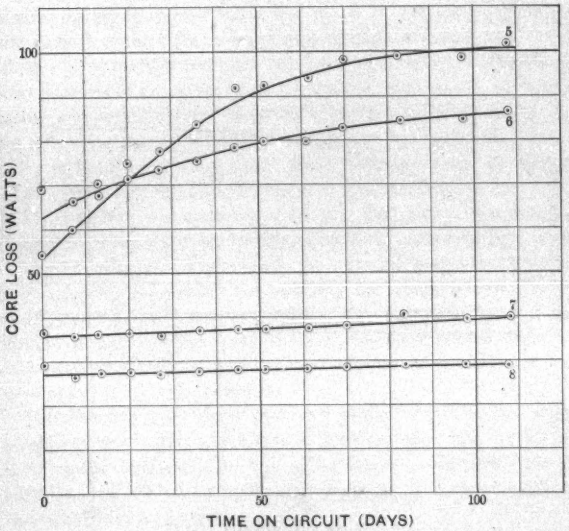

FIG. 2.

The first value of the core loss is high because it was made while the transformers were at the temperature of the room, while the other readings were taken while they were hot. In calculating the increase in the loss, the results of a previous core-loss measurement were used in order to get the total aging; for a slight increase was noticed on account of the use noted previously. It is to be noted that while the increase is in the hysteresis loss, the results are given for the total loss which will make the percentage increase of the hysteresis loss somewhat greater.

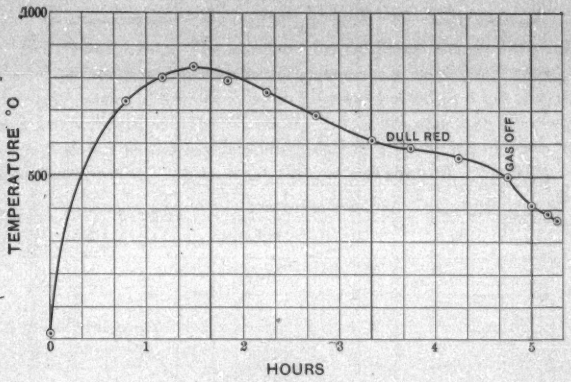
1000
500
TEMPERATURE °O
DULL RED
GAS OFF
0
1
2
3
4
5
HOURS

Fig. 3.

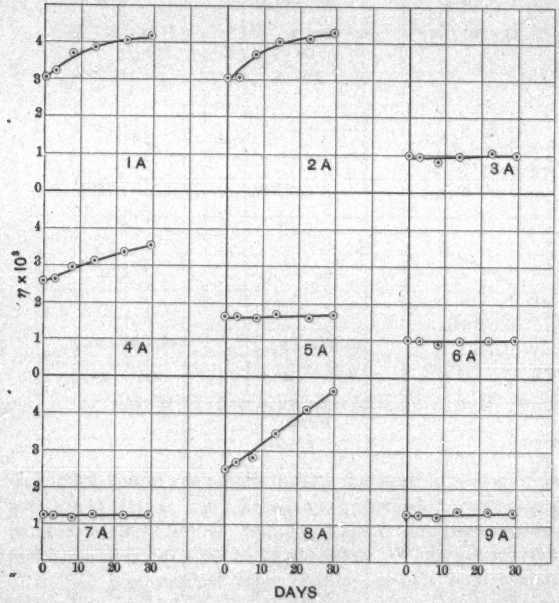
1 A
2 A
3 A
4 A
5 A
6 A
7 A
8 A
9 A
η × 10⁴
0
10
20
30
DAYS

Fig. 4.

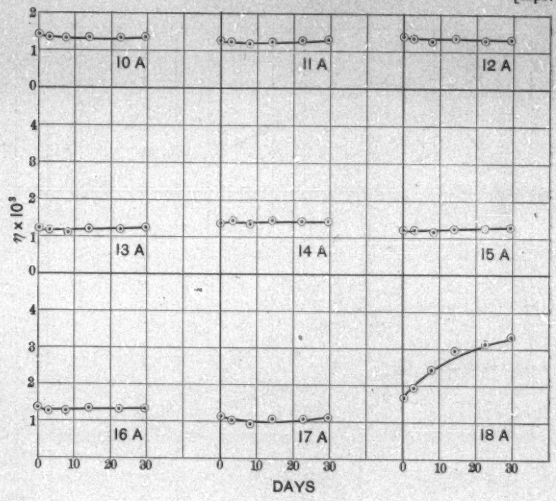
10 A
11 A
12 A
13 A
14 A
15 A
16 A
17 A
18 A
$\eta \times 10^3$
DAYS

Fig. 5.

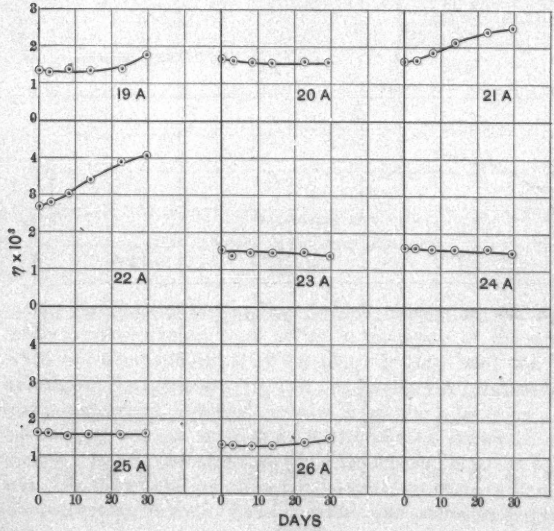
19 A
20 A
21 A
22 A
23 A
24 A
25 A
26 A
$\eta \times 10^3$
DAYS

Fig. 6.

II.—Test of the Hysteresis Loss of Small Samples.

Heat Treatment.—The samples were subjected to four different heat treatments which will be designated A, B, C, and D throughout this paper. Samples A, B, and C were heated in a gas muffle to a temperature of 835° C. taking 1.5 hours, and then allowed to cool slowly as the muffle cooled, taking 3.75 hours to cool to a temperature of 365° C. when the gas was shut off. Samples D

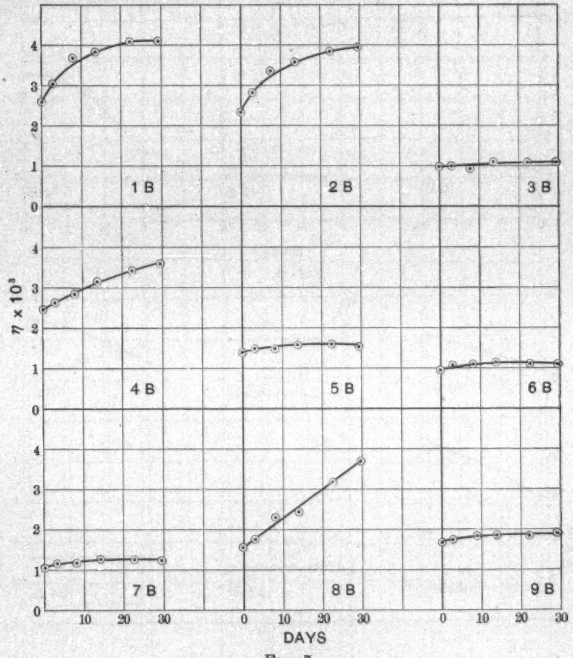

Fig. 7.

were taken out of the muffle during the heating, when a temperature of 804° C. was reached, and quenched in water at a temperature of about 15° C. Samples B and C were heated with the muffle the next day after being annealed. When a temperature of 205° C. was reached after heating one-half hour, samples B were taken out and quenched. After one hour, when a temperature of 400° C. was reached, samples C were quenched.

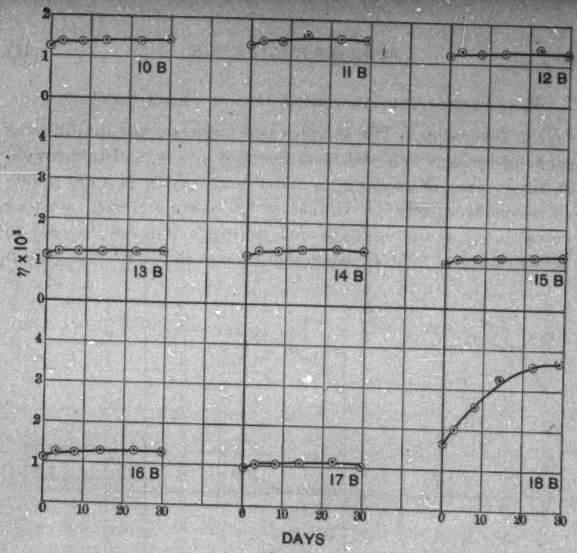
10 B
11 B
12 B
13 B
14 B
15 B
16 B
17 B
18 B
η x 10³
DAYS

FIG. 8.

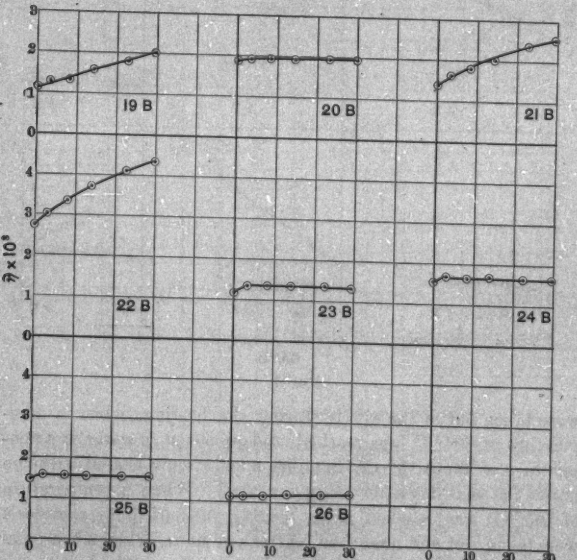
19 B
20 B
21 B
22 B
23 B
24 B
25 B
26 B
η x 10³
DAYS

FIG. 9.

The temperature of the gas muffle used for annealing was measured by means of a resistance pyrometer using a platinum wire having a resistance of 14.78 ohms at 0° C. and 18.64 ohms at 100° C. The temperature is deduced from the resistance of the pyrometer by means of the following formula.[7]

$$t = 100\,\frac{R - R_0}{R_t - R_0} - \delta\left[\left(\frac{t}{100}\right)^2 - \frac{t}{100}\right]$$

For commercial platinum

$$\delta = 1.57 + 15\left(1.3383 - \frac{R_t}{R_0}\right)$$

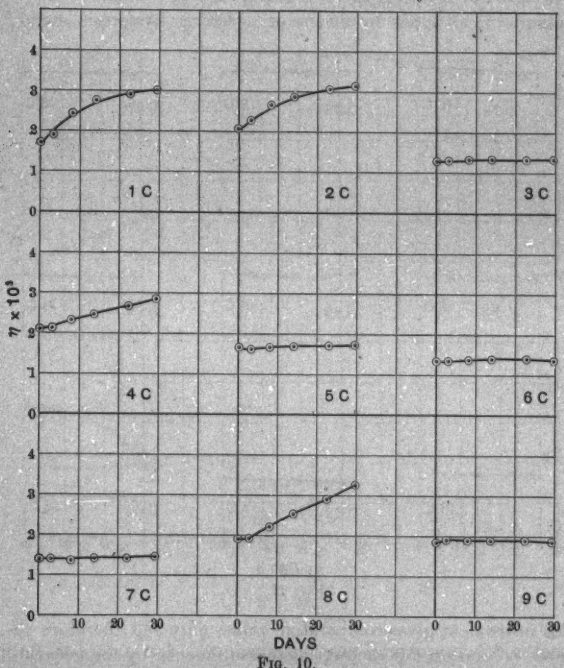

FIG. 10.

where t is the temperature and R_0, R_t and R are the resistances at the temperatures 0°, 100° and t° C respectively. Values were assigned to t, the equation solved for R_t, and the results plotted in a curve showing the relation of t to R_t, from which the tem-

7. "On the Practical Measurement of Temperature." H. B. Collander. *Phil. Trans. Royal Soc.* of London. Vol. 178, p. 161.

perature of the muffle was obtained from the known resistance of the pyrometer.

The relation of the temperature to the time during the first heating and cooling is shown in Fig. 3.

After tying up the specimens in shape for measuring their hysteresis loss, they were placed in a box which had an average temperature of 60° C, with a variation of about 5° C each way, and kept there for a period of 29.5 days; their hysteresis loss being measured from time to time.

Measurement of Hysteresis Loss.—The hysteresis loss was measured in all cases by means of a Ewing hysteresis meter.[8]

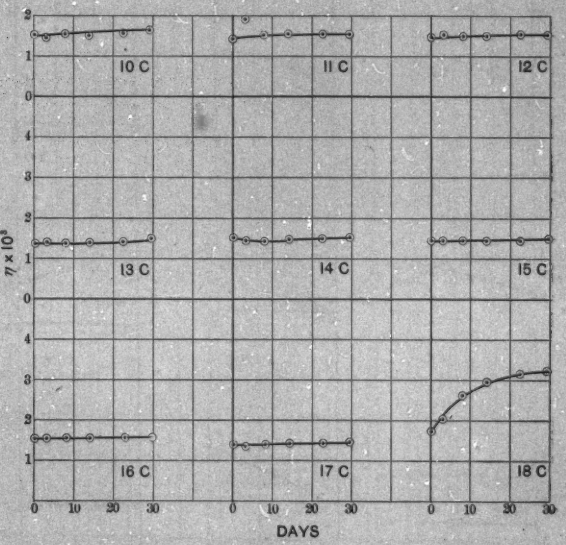

FIG. 11.

This instrument gives comparative values only and therefore requires calibration, for each set of measurements, by the insertion of two standard samples. These samples are sent with the instrument and their hysteresis loss as given is assumed to be correct. Results obtained with this instrument are likely to be in error as much as 3%, consequently two specimens of each sample

8. "A Magnetic Tester for Measuring Hysteresis in Sheet Iron." *Electrical Engineer*, (London). Vol. 29, p. 437.

were taken and the mean value of their hysteresis loss was used in plotting the curves and computing all results.

The hysteresis loss was measured six times during the 29.5 days that the specimens were kept hot. Owing to difficulties due to imperfect heating apparatus and the necessity of allowing the specimens to cool while their hysteresis loss was being measured,

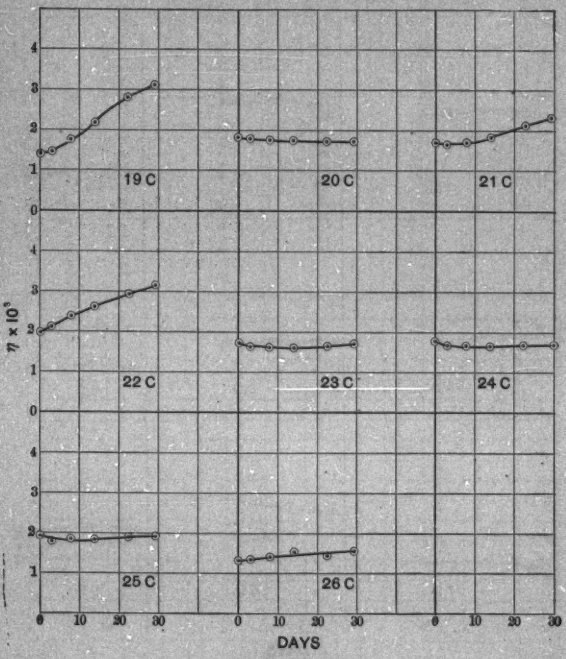

FIG. 12.

the heating was not continuous; but was interrupted for several hours at various times.

A preliminary test of 15 days, which is designated by the letter E, was made on the samples as received. The temperature for this test was 80° C, which was reduced to 60° C for the final test because after correspondence with transformer manufactu-

rers the former temperature was thought to be too much above that normally found in good transformers.

The results of the tests are shown in Table II, and Figs. 4 to 18 inclusive, and are given in terms of Steinmetz's hysteresis constant obtained from the equation $H = \eta B^{1.6}$. H is the hysteresis loss in ergs per cu. cm. per cycle; η the hysteresis constant;

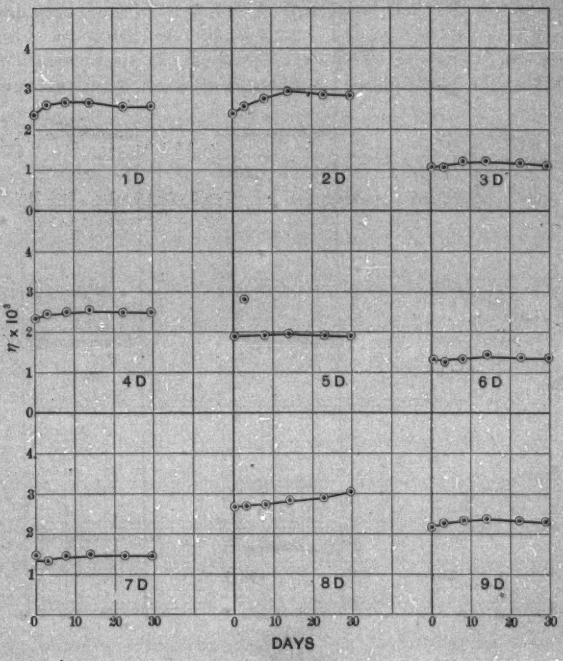

FIG. 18.

and B the magnetic density in gausses. For convenience in writing, the constant is multiplied by 10^3 in the results.

Chemical Composition.—The samples were analyzed to find the amount of silicon, phosphorous, manganese, sulphur and combined carbon which they contained. Table IV shows the results of this analysis.

TABLE III.

INITIAL HYSTERESIS CONSTANT $\times 10^3$.

HEAT TREATMENT.

No.	A.	B.	C.	D.	E.
1	3.05	2.60	1.70	2.35	1.72
2	3.03	2.35	2.10	2.40	3.24
3	.90	1.00	1.32	1.05	.90
4	2.55	2.45	2.15	2.40	3.24
5	1.58	1.45	1.68	1.92	1.87
6	.98	.93	1.30	1.25	.98
7	1.22	1.08	1.38	1.40	1.21
8	2.45	1.60	1.90	2.08	4.39
9	1.30	1.70	1.92	2.20	2.01
10	1.40	1.35	1.57	1.75	1.43
11	1.22	1.40	1.46	1.40	1.43
12	1.34	1.30	1.45	1.50	1.31
13	1.18	1.25	1.40	1.35	1.35
14	1.40	1.25	1.45	1.50	1.33
15	1.18	1.15	1.45	1.50	1.16
16	1.30	1.20	1.50	1.50	1.47
17	1.02	1.00	1.40	1.43	1.14
18	1.65	1.62	1.75	1.85	2.33
19	1.35	1.20	1.40	1.55	1.53
20	1.02	1.08	1.80	3.08	3.26
21	1.70	1.40	1.65	1.75	1.95
22	2.74	2.70	2.60	2.40	3.51
23	1.38	1.35	1.65	1.30	
24	1.60	1.65	1.68	1.30	
25	1.54	1.12	1.85	1.95	
26	1.30	1.15	1.45	1.35	

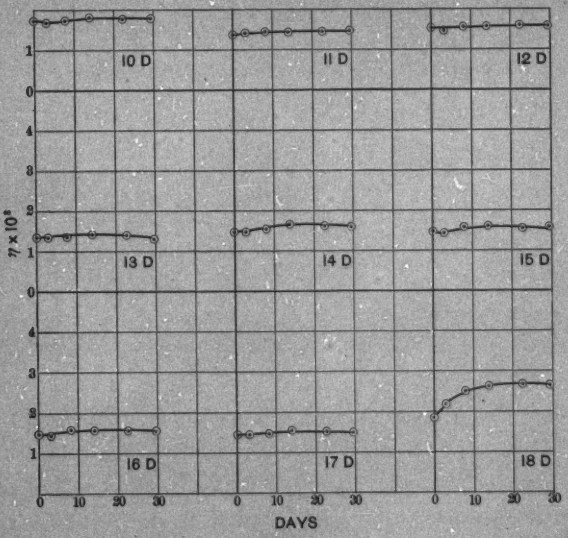

FIG. 14.

TABLE III.

FINAL HYSTERESIS CONSTANT $\times 10^3$.

HEAT TREATMENT.

No.	A.	B.	C.	D.	E.
1	4.15	4.12	3.05	2.37	3.52
2	4.21	3.94	3.18	2.84	4.12
3	.99	1.09	1.37	1.15	1.02
4	3.54	3.61	2.88	2.51	3.90
5	1.69	1.54	1.74	1.91	1.86
6	1.03	1.10	1.39	1.31	.98
7	1.99	1.81	1.44	1.42	1.88
8	4.50	3.71	3.26	3.08	4.51
9	1.38	1.92	1.89	2.32	1.95
10	3.36	1.48	1.65	1.81	1.59
11	1.26	1.49	1.53	1.44	1.44
12	1.41	1.26	1.54	1.56	1.31
13	1.23	1.26	1.50	1.35	1.25
14	1.40	1.32	1.57	1.60	1.30
15	1.25	1.26	1.51	1.60	1.15
16	1.36	1.31	1.59	1.55	1.56
17	1.22	1.04	1.48	1.50	1.15
18	3.43	3.71	3.25	2.65	3.01
19	1.76	2.09	3.13	2.15	2.74
20	1.49	1.08	1.71	3.22	2.26
21	2.64	2.37	2.51	2.32	2.74
22	4.15	4.35	3.20	2.95	4.39
23	1.43	1.32	1.70	1.86	
24	1.41	1.65	1.70	1.81	
25	1.60	1.61	1.93	2.00	
26	1.44	1.76	1.56	1.38	

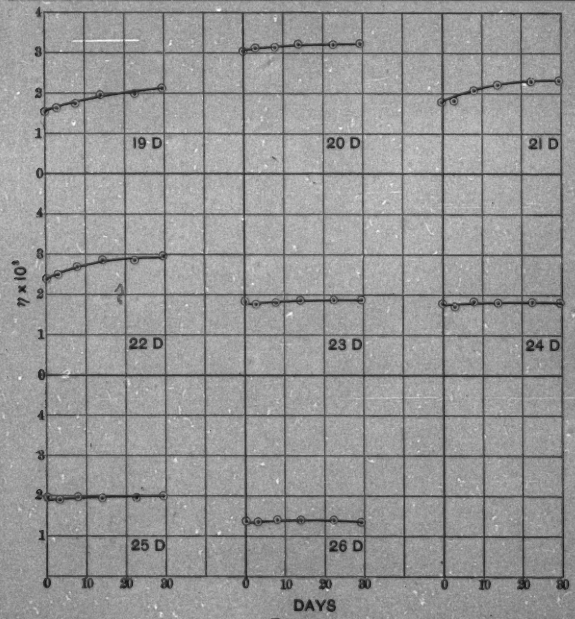

FIG. 15.

TABLE III.

INCREASE IN HYSTERESIS CONSTANT % AFTER 29.5 DAYS AT 60° C.

HEAT TREATMENT.

No.	A.	B.	C.	D.	E.
1	36	58	79	9	1·5
2	39	68	51	18	27
3	3	9	4	9	13
4	30	47	34	5	13
5	7	6	4	−1	−1
6	5	18	7	5	0
7	3	12	4	1	6
8	84	132	71	15	3
9	6	13	−1	5	−3
10	142	10	5	4	11
11	3	6	5	3	−1
12	5	−3	0	4	0
13	4	1	7	0	−7
14	0	6	8	7	−2
15	6	10	4	7	−1
16	5	9	6	3	6
17	20	4	6	−5	1
18	108	130	86	45	29
19	30	74	123	38	72
20	−8	3	−5	5	0
21	55	83	40	33	40
22	51	61	60	23	24
23	4	−2	3	43	
24	−12	0	1	30	
25	4	6	4	−3	
26	11	10	8	2	

TABLE IV.

IMPURITIES IN IRON SAMPLE %.

No.		Si.	P.	Mn.	S.	C. C.
1	Swede Iron	Trace	.028	Trace	.02	.05
2		.066	.120	.16	.05	.05
3	Open hearth steel	Trace	.027	.30	.05	.06
4		.056	.125	.33	.025	.06
5	Steel	Trace	.105	.44	.02	.06
6		"	.059	.27	.02	.05
7		"	.092	.36	.03	.05
8	Bessemer steel	"	.089	.42	03	.08
9		"	.093	.47	.02	.07
10		.028	.089	.25	.025	.05
11		Trace	.056	.38	.03	.05
12	Steel	.017	.071	.37	.018	.10
13	"	.020	.073	.41	.025	.10
14	"	.020	.076	.29	.030	.10
15	"	.016	.060	.37	.02	.10
16		Trace	.023	.52	.05	.07
17		.006	.075	.43	.03	.07
18	Iron	.048	.210	.10	.02	.06
19	Beasemer Steel	Trace	.109	.39	.06	.05
20	" "	.009	.064	.39	.02	.09
21		Trace	.093	.30	.07	.06
22		.049	.085	.34	.02	.07
23		Trace	.099	.36	.03	.07
24		"	.071	.34	.02	.05
25		"	.074	.27	.02	.09
26		"	.101	.26	.03	.05

Discussion of Results.

In order to make the relation, if one exists, between the hysteresis phenomena and the impurities in the iron clearer, three sets of results were plotted. These show the relation between (1.) the various impurities, taken one at a time, and the initial hysteresis constant; (2.) the various impurities, taken one at a

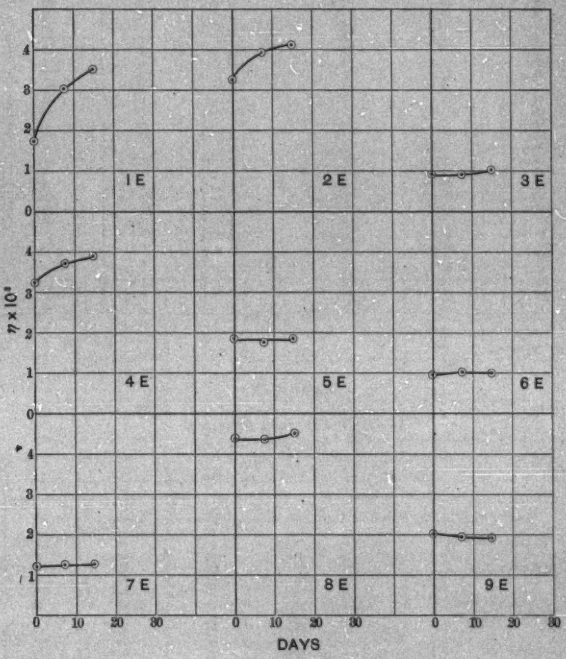

Fig. 16.

time, and the increase in the hysteresis constant; and (3.) the various impurities taken two at a time and the final hysteresis constant. Samples of these results as plotted are shown in Figs. 19 to 21 inclusive. The relation between the final hysteresis constant and the sum of the impurities was also plotted. Fig. 22

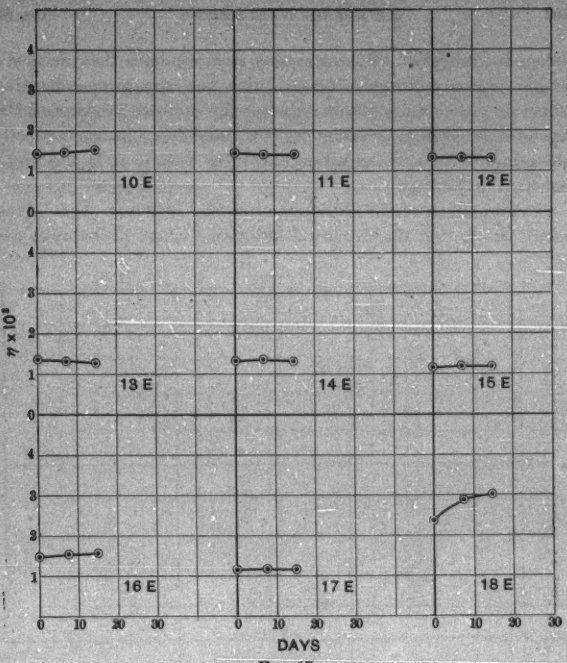
10 E
11 E
12 E
13 E
14 E
15 E
16 E
17 E
18 E
η x 10⁵
DAYS

FIG. 17.

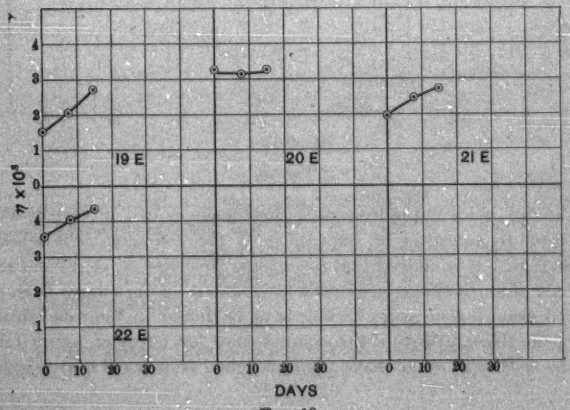
19 E
20 E
21 E
22 E
η x 10⁵
DAYS

FIG. 18.

From an inspection of these results it is evident that neither a single impurity nor pair of impurities has any preponderating effect on either the absolute value or the increase in value of the hysteresis constant. Specimens No. 7 and No. 8 have practically the same chemical composition but differ widely as to hysteresis constant and aging.

A comparison of the constants after the different heat treatments shows that the structural difference which probably causes

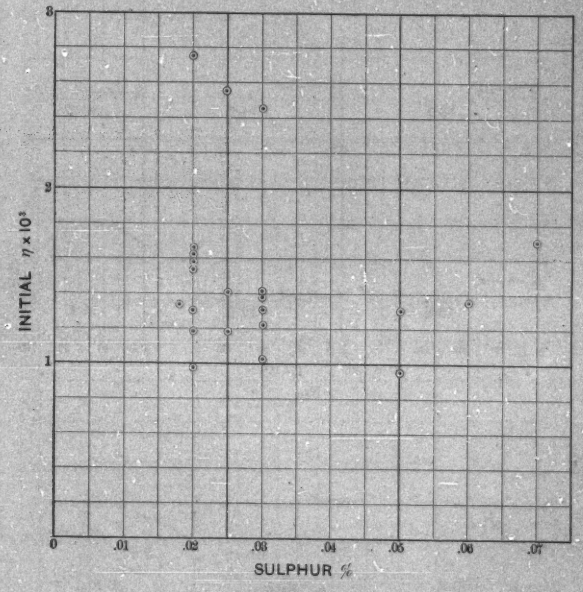

FIG. 19.

the difference in the hysteresis phenomena is not materially affected by annealing or quenching if the temperature is below 850° C. It is noticed that specimens, Nos. 1, 4, 8, 18, 21 and 22 have their final hysteresis constant reduced by quenching from a red heat; but no cause for this is to be found in their chemical composition. Most of the samples show less effect of aging for the specimens which have been quenched from a red heat, which

is probably due to the iron being in a more rigid state after such treatment and consequently allowing less change to take place at the comparatively low temperature of 60° C.

The best sample tested is No. 3 which is of open hearth steel and is nearly equaled by Nos. 6 and 7 of which the process of working is unknown.

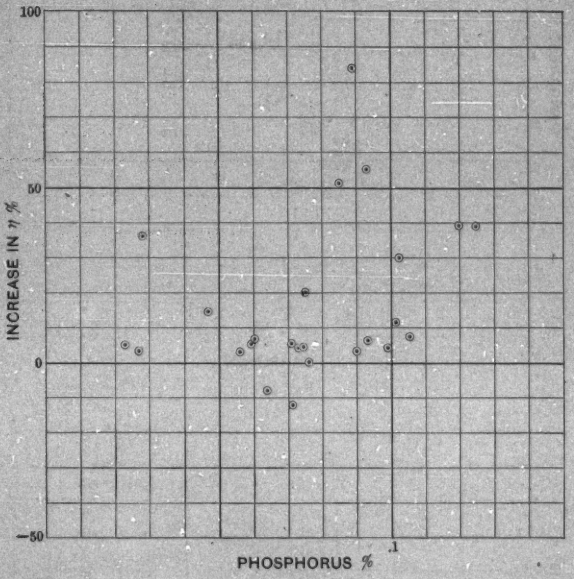

FIG. 20.

By an inspection of the curves showing the relation between the hysteresis constant and time, it is seen that several of the samples show an increase and then a decrease in the hysteresis constant; as is shown more plainly when the iron is kept at a higher temperature.[9]

9. "The Aging of Transformer Iron." R. C. Clinker, *Electrician*, (London), Vol. 41, p. 291.

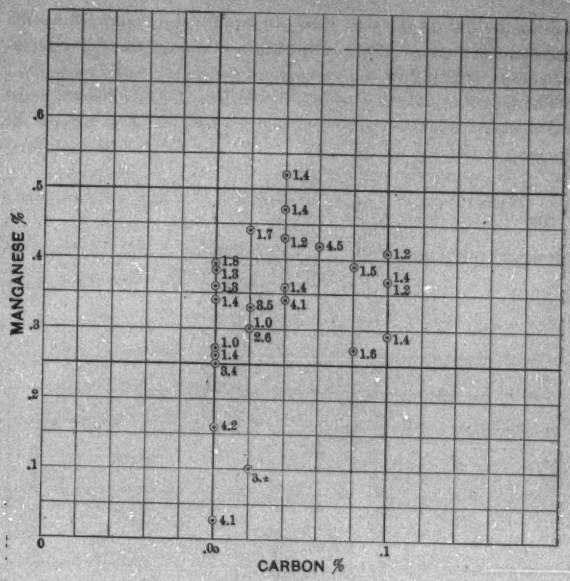
MANGANESE %
CARBON %
0
.05
.1
.2
.3
.4
.5
.6
1.4
1.4
1.7
1.2
4.5
1.2
1.8
1.3
1.3
1.4
1.4
1.5
1.4
1.2
3.5
4.1
1.0
2.6
1.0
1.4
3.4
1.6
1.4
4.2
3.+
4.1

FIG. 21.

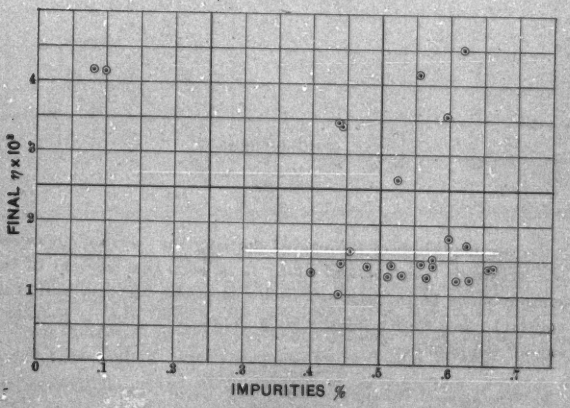
FINAL $\eta \times 10^3$
IMPURITIES %
0
1
2
3
4
.1
.2
.3
.4
.5
.6
.7

FIG. 22.

Conclusion.

The conclusion to be drawn from this research is that the hysteresis constant for iron as pure as the samples tested is not so much dependent upon the chemical composition as upon the physical structure of the material due to the method of working. The most hopeful direction for future research in this subject seems to be by watching the metal in all stages of its manufacture and studying its microscopic structure.

ALEXANDER STRATTON.

Born November 14th, 1872.
Died October 30th, 1899.

ALEXANDER STRATTON was born in New York City, on November 14th, 1872, and was educated in the public schools, taking a full mechanical course in the College of the City of New York, graduating in 1892. He took a post-graduate course in electrical engineering at Columbia University, graduating in 1894. He was elected an Associate Member of the INSTITUTE, March 20th, 1895.

After graduation, he spent two years with the Crocker-Wheeler Company, at their works, in Ampere, N. J., and in 1896, entered the employ of the "C and C" Electric Co., at Garwood, N. J., as a designer of machinery.

In 1898 he had an attack of pleurisy, which greatly weakened him, and during the winter of 1898–9, he contracted a severe cough, which growing steadily worse developed into consumption, so that in the spring of 1899 he was compelled to give up work, to a considerable extent, and endeavor to recuperate. He went to Liberty, N. Y., where his health temporarily improved, but the loss of his little child caused a relapse and he went to Colorado. Finding the altitude too great, he returned to Liberty in September, where he died on October 30th, 1899.

Mr. Stratton was an able, faithful engineer, continuing his regular duties long after his physical condition demanded a rest. While enjoying the confidence of his employers, he was also beloved by all who were in any way associated with him, and his cheery presence will be missed by many.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, January 24th, 1900.

The 139th meeting of the INSTITUTE was held this date at 12 West 31st Street, and was called to order by President Kennelly at 8.25 P. M.

THE SECRETARY:—At the meeting of the Council this afternoon, the following associate members were elected:

Name.	Address.	Endorsed by
BEHREND, BERNHARD A.,	Erie, Pa.	C. P. Steinmetz. T. C. Martin. W. D. Weaver.
BEVERIDGE, EDMUND WALTER,	Assistant Engineer, The G. I. P. Railway Co., India, Sirud Khandish District, Bombay, India.	Ralph W. Pope. N. S. Keith. F. E. Kinsman.
DENHAM, JOHN	Electrician, Cape Government, Cape Town, South Africa.	W. M. Mordey. J. E. Lloyd. A. E. Worswick.
EVANS, PAUL H.	Chief Engineer, Mexican General Electric Co., Apartado 403 Mexico City.	C. F. Beames. A. E Worswick. John W. Thompson
GREENWOOD, GEORGE,	Electrical Engineer and Superintendent, Jalapa Railway and Power Co., Jalapa, V. C. Mexico.	John W. Thompson C. F. Beames. A. E. Worswick.
HOLMES, GWYLLYM R.	Holmes-Rose Electric Co., 2842 Parkwood Ave., Baltimore, Md.	H. K. McCay. Gano S. Dunn. M. C. Schwab.
HYDE, J. E. HINDON,	120 Broadway; residence, 48 West 11th St., New York City.	C. F. Chandler. C. A. Doremus. Henry Morton.
MAGNUS, BENJAMIN	Electrical Engineering Student, Columbia University; residence, 22 E. 111th Street, New York City.	Geo. F. Sever. Fitzh. Townsend. F. B. Crocker.
NILES, HARRY B.	Electrical Engineer, Ferrocarrillos del Distrito, American Club, Mexico City.	John W. Thompson C. F. Beames. A. E. Worswick.

PFEIFFER, ALOIS J. J.	Engineer, Thomson - Houston Co., 5 Piazza Castello Milano, Italy.	H. A. Foster. W. J. Davis, Jr. A. H. Armstrong.
POOLE, CHARLES OSCAR	General Superintendent Electrical Dept. San Francisco Gas and Electric Co,, 229 Stevenson St.; residence, 452 Bryant St., San Francisco, Cal.	J. A. Lighthipe. Geo. P. Low. F. A. C. Perrine.
POWELSON, WILFRED VAN NEST,	Government Inspector, Electrical Appliances, General Electric Co., Schenectady, N.Y.; Lieutenant U. S. Navy.	Eskil Berg. Ernst Berg. Chas. P. Steinmetz
SAYLOR, FREDERICK ALEXANDER.	Chief Electrician, U. S. S. Chicago; residence, Reading, Pa.	W. E. Chappell. Arthur L. Rice. T. P. Thompson.
SELDEN, R. L. JR.,	Deep River, Conn.	F. G. Strong. R. W. Pope. T. C. Martin.
SHEARER, J. HARRY	Electrical Engineer, National Electric Light Co., Apartado, 639 Mexico City, Mexico.	John W. Thompson C. F. Beames. A. E. Worswick.
SHEPHARD, ROBERT R,	Erecting Engineer, Mexican General Electric Co., Apartado 408, Mexico City, Mexico.	C F. Beames. A. E. Worswick. John W. Thompson
THOMPSON, MILTON T.	Constructing Engineer, Mexican General Electric Co., Apartado 408, Mexico City, Mexico.	C. F. Beames. A. E. Worswick. John W. Thompson
ZABEL, MAX W.	Draughtsman and Student of Patent Law with John A. Brown & Cragg, 1450 Manadnock Bg.; residence, 454 North Ave., Chicago, Ill.	D. C Jackson C. F. Burgess M. C. Beebe.

Total 18.

The following associate members were transferred to membership:

Approved by Board of Examiners, September 7th, 1899.

WALTER DOUGLAS YOUNG, Electrical Engineer, B. & O. R. R., Baltimore, Md.

Approved by Board of Examiners, October 20th, 1899.

LOZIER, ROBERT T. E. Electrical Engineer, Member of Firm of Bullock Electric Co., St. Paul Bldg., New York City.

Approved by Board of Examiners, Dec. 8th, 1899.

H. A. STORRS, Assistant Engineer, U. S. Engineer's office, New London, Conn.

THE PRESIDENT:—Since our last meeting the INSTITUTE has suffered by the death of two of our well-known members, Mr. James Hamblet has served the INSTITUTE for some years, both as vice-president and manager. His contribution to the electrical interests, work and fraternity extend over half a century.

Mr. Dana Greene had rendered valuable and active service to the INSTITUTE, not only as Manager on the Council, but also in contributing papers. By his extensive general knowledge and genial influence he greatly promoted electrical engineering progress in those branches which he made peculiarly his own. His untimely death will be greatly deplored not only by all our members who knew of his work, but by the favored few who were acquainted with his charming personality. The Council has at its meeting to-day passed resolutions to be communicated to the families of these gentlemen, and has ordered suitable obituary notices to be printed in the TRANSACTIONS.

The following paper was then read by Mr. Fitzhugh Townsend on "A New Method of Tracing Alternating Curves."

[See January issue p. 75.]

Discussion.

Dr. Samuel Sheldon:—I have been very much interested in reading this paper and hearing the description of this ingenious piece of apparatus. But it seems to me that in order that the instrument may give directly the curve of induction, that the two halves of the cycle of alternation must be perfectly symmetrical; that is, the positive flux of current must have the same time relation that the negative flux has. I cannot see from an examination of the mathematical equation that there is any difficulty with the equation. But a general equation should, if correct, be applicable to every case. It does not seem to apply to the case of the exaggerated curve shown in Fig 1, or at least, the contact-maker would not yield the correct curve of induction. Let the rectangular curve represent the time relation of the E. M. F. With proper arrangement of apparatus this will also be the curve of

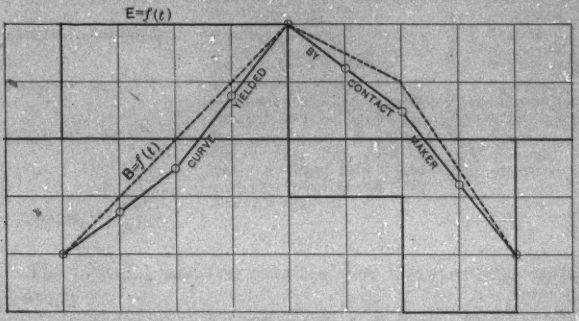

Fig. 5.

current. The relation which must exist between the induction and the time, in order to produce the curve of E.M.F., is represented by the dotted curve. The dotted curve is the integral curve of the E. M. F. curve. Now the curve which would be yielded by the contact-maker as described in the paper, the contact being maintained for half a cycle, would not coincide with this dotted curve of induction. The voltmeter readings will be proportional to the algebraic sum of the areas lying between the E. M. F. curve, the time axis, and the time intercepts of making and breaking contacts. A slight inspection will show that the curve passing through the points enclosed in circles would be the one which would be yielded by the contact-maker.

The difficulty in using a telephone and contact-maker with, say, the Mershon method is two-fold. In the first place the ordinary frequencies of alternating currents give a musical tone by

means of the contact-maker, to which the ear is not particularly sensitive, and it is difficult to distinguish the intensity of a minimum, providing one cannot get absolute silence. In the second place, with a contact-maker having any breadth of contact at all, it is impossible to balance a variable E. M. F. against a constant E. M. F. Therefore, unless the contact's breadth be infinitesimal silence cannot be obtained. If, however, one uses a contact of sufficient breadth to be sure that the contact is made with every revolution of the contact-maker (of breadth, not necessarily to half a cycle), and makes use of the voltmeter as described in the paper *i. e.*, really as a ballistic galvanometer, then one can get a curve such as is yielded by an ordinary contact-maker and a very satisfactory one, without the liability of the contacts getting out of order.

In this use of the Weston voltmeter we have still another important adaptation of that valuable instrument. I have had occasion to use a Weston voltmeter for ballistic work, and it perhaps may be of interest to some who may not know it, that the Weston voltmeter will give deflections or throws in response to instantaneous quantities of electricity passing through it which are directly proportional to these quantities. Now a ballistic galvanometer has been defined by a good many different people as a galvanometer which has a needle which has a large moment of inertia and which has a long period of swing.

DR. PUPIN:—And small damping.

DR. SHELDON:—That is sometimes put in, yes.

DR. PUPIN:—Always.

DR. SHELDON:—Well, all right. It does not make any difference. It is not necessary that the ballistic galvanometer should have these qualities. If we calibrate a Weston voltmeter in a proper manner it can serve the purposes of a ballistic galvanometer just as well as the old fashioned kind. Those who have ever had occasion to take hysteresis curves by the step-by-step method, using a galvanometer which has a long swing of from 12 to 15 seconds, with two or three minutes' wait before the needle comes to rest after each swing, know what a tedious process it is to get a hysteresis curve. Yet the hysteresis curve can be taken with a Weston voltmeter of the ordinary round pattern switch-board style, having its series resistance removed, and it can be taken in less than two minutes, with a sufficient number of points to give accurate results, if the curve is properly integrated.

MR. FITZHUGH TOWNSEND:—With regard to that objection about determining the curve of induction, I have never attempted to determine the curve of induction from an unsymmetrical electromotive force. In all practical machines, good machines, and almost all alternating current machines, the positive half of the wave is the same as the negative half of the wave, and in that case I think the method holds good. A slight modification of the me-

thod which I have not yet tried, which would involve the use of two brushes, would I think obtain a symmetrical curve of induction, but that is more or less a *tour de force*. I have, therefore, only devoted my attention to the treatment of curves which were symmetrical with respect to the zero line.

Mr. Charles P. Steinmetz:—During late years no end of different methods have been brought out to take instantaneous readings of electrical quantities, mostly differing from each other by a different kind of contact-maker or a different material used, or other such minor qualities, or an improved method of using it. Now the method brought before us to-day essentially differs from previous methods, as far as I know them, by, I believe very favorable features. In its general principle this method described here consists of the following: To get instantaneous readings of a quantity, we derive from this quantity another quantity, which is a differential of the former, and take integral readings, that is average values of the latter quantity. We thereby get rid of most of the difficulties incident to instantaneous readings. We are independent of the width of the contact. We are independent of the impossibility of getting an absolute point-contact of no duration, and of the difficulties due to the arc following the contact-brush for some appreciable time. There are some limitations to this method, which after they are pointed out will probably be overcome. The source of error shown in Fig. 2 as due to the current not assuming instantly the full value, I do not consider so essential, because there is another error compensating for it. In Fig 2, the current in the moment when the circuit is broken, does not instantly drop to zero, but gradually dies out by a small arc continuing to carry the current. This gradual dying-out of the current compensates for the error mentioned in the paper to a certain extent. If you think of it still more, the error introduced by the current not stopping instantly, depends on the voltage at which you operate, and gets larger with increase of voltage, while the error due to the current not instantly reaching full value gets less with increase of voltage, thus theoretically you can always find a voltage, where the errors just neutralize each other. Practically, therefore, the method should give very accurate results provided that the errors are small enough *per se*, so as not to require perfect compensation.

The principle of this method as brought out here, however, can be applied in many different ways. It is shown only for determining the induction curve of the machine. This I do not consider as so very important, because you can get this curve of induction, although a little more laboriously, from the electromotive force curve; but you can apply the same method wherever you want instantaneous values of a quantity, and are able to get differential values of this quantity. To illustrate, for instance, one of the problems of great importance at present is to determine the variation of the rate of rotation of flywheels of slow-

speed engines which are intended for use with synchronous apparatus as alternators, synchronous motors, etc. What you want to get there is the instantaneous displacement of the flywheel from the position it would have at uniform rate of rotation. Now the differential of this displacement is the velocity of the flywheel. Assume that on the shaft of the flywheel you have a small dynamo with constantly excited field, the field, for instance, excited by a storage battery. Take average or integral values of the electromotive force given by this dynamo, then you see these integral values give you direct the position of the flywheel. You would in such a case probably counterbalance the largest part of the electromotive force of the small dynamo by a storage battery with a constant electromotive force, to get a larger variation relatively than the very small variation of speed, and you would have to develop this method like you would have to develop any method.

But I believe that integrating or averaging the differential values of the quantity of which you want instantaneous values can be applied to many cases, and in some cases which are probably more important than the determination of instantaneous values of electromotive force, since there are no suitable methods in existence for such other cases.

DR. M. I. PUPIN:—I do not think that Dr. Sheldon's objection is a very serious one, for the reason that Mr. Townsend already pointed out. The method which Mr. Townsend describes is intended to analyze curves that are obtained in ordinary machines, well designed, not badly designed machines. If a machine is badly designed it is unworthy of investigation; the less you fuss with it the better. But even with a badly designed machine such as that would be which Dr. Sheldon presented on the blackboard, by a slight modification of the method as Mr. Townsend has already indicated, using two brushes, the difficulty could be gotten around. In all our commercial machines the electromotive forces and the currents are perfectly symmetrical on the two sides of the axis of the abscissæ. In that case the method will work all right. Now so far as any errors are concerned, I think that Mr. Townsend has rather enlarged them or made them appear more serious than they really are,—the source of error, namely the time constant. The time constant comes in the curve not rising instantaneously to its full value when you close the circuit and not falling off to zero when you break the circuit. That does not amount to much, because for other reasons you have to make the resistance large in comparison to the inductance of the circuit. In that case of course you have the time constant, a very large quantity; the current will in a very small fraction of the half period come to its full value. So that error really does not amount to anything appreciable. It certainly is very much smaller than other errors of observation that we meet with in investigations of this kind. The errors of

observation in investigations of this kind are nearly one per cent. If any body does as well as that he is fortunate, and this error introduced by having the time constant a little bit too small will not be anything more than a very small fraction of one per cent.

Mr Steinmetz has said that we do not care so very much for the curve of quantity. Well, I think we do. You see in our present measurements we have three different kinds of instruments, fundamentally different. We have the voltmeter and the ammeter and the ballistic galvanometer—not in the way that Dr. Sheldon defined it, but in the way that it ought to be defined. The one measures the rate of variation of the quantity of electricity; that is the ammeter. The other measures in dynamo-electric machines the rate of variation of a magnetic quantity, this is the voltmeter; and the third one, the ballistic galvanometer, measures the quantity, either the quantity of electricity or the quantity of magnetism. Now in this method that Mr. Townsend brought before us to-night, we have another method which resembles the ballistic, to be sure, but still it is a different story from the ballistic, altogether a different story —measuring quantities, measuring the quantity of electricity and the quantity of magnetism directly, not in a roundabout way by integrating a curve. Whoever has made the calculcations of integrating a periodic curve knows that there are a great many sources of error, very much greater sources of error than you meet with in experimental work of this kind. Some of us might say; what is the use of measuring the quantity of magnetism or the quantity of electricity? There are investigations and that too not only of purely scientific interest but also of very great commercial and engineering value in which we do care for quantity of magnetism, quantity of induction and quantity of electricity.

I mention this for the purpose of bringing out something which Mr. Townsend has not brought out. This method was evolved in our laboratory in connection with an investigation which should be mentioned here. He has worked now for over a year very faithfully over a difficult problem—the problem of determining, what I choose to call, the dynamical hysteresis loop.

If you will allow me, I will point out the importance of this loop. If you determine an alternating electromotive force curve, and the corresponding current curve by a sliding contact, or any other way, and from the secondary E. M. F. curve, determine the curve of induction in a transformer, and then you plot a curve of Fig. 6, taking from the abscissæ the ampere turns in the primary, for the ordinates you take the total induction B of the transformer. You get the curve of Fig. 6 which I call the dynamical hysteresis loop. Its area represents the hysteresis and Foucault current loss per cycle. That is the total loss in the transformer, provided, of course, the secondary is not closed. But if the secondary is closed, then that is the total work done in the trans-

former, taking in hysteresis, Foucault current primary I^2R and secondary load.

Well, you see, the determination of curve in Fig. 6 is a very important thing. It enlarges our idea of the hysteresis loop. If you have no secondary current then you can study exactly what is going on in the iron while it is being magnetized and demagnetized. If you have a very high degree of lamination there are no appreciable Foucault current losses at ordinary commercial frequencies and in accordance with investigations which have been done abroad (and we have done some work of this kind in Columbia University owing principally to the per-

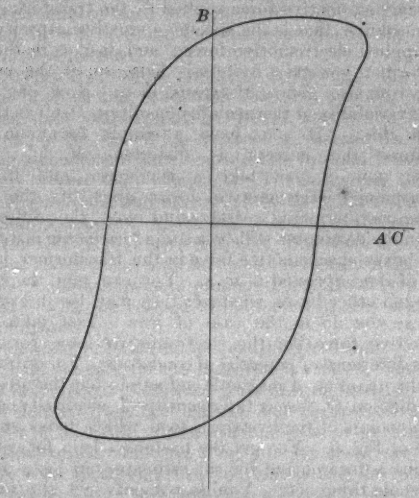

FIG. 6.

servance and exertions of Mr. Townsend) then the hysteresis loop of Fig. 6 becomes more nearly like the statical hysteresis loop, determined with direct current and very slow cycles. If you magnetize the iron very slowly, the curve develops sharp corners as you all know. But you probably never get corners with alternating currents. Now, of course, if the impressed primary electromotive force is a simple harmonic, the induction will be a simple harmonic, and we can derive the induction curve very easily from the curve of the secondary electromotive force. There is no difficulty at all in doing that. But if the primary impressed electromotive force is a complex harmonic, the induction will be

a complex harmonic, and if you try to deduce the induction from the secondary electromotive force you have to go through a lot of most tedious calculations. Mr. Townsend's method avoids this completely. It saves, therefore, a tremendous amount of labor and liability of error.

There is another thing that we have been trying to do for years and could not get it. How could this extended idea of the hysteresis loop be applied just as well to the losses in condensers? The reason that we get a hysteresis loop which is not an ellipse is because the permeability of the iron is a variable quantity. Now if you take a condenser you will find that the condenser capacity varies with the voltage just as the permeability varies with the magneto-motive force applied to the transformer. The static permeability, that is the specific inductive capacity, varies with the applied electromotive force; and just as in the case of the transformer, you get a hysteresis loop, so in the case of a condenser you also get a hysteresis loop. Now one way in which this variability or permeability manifests itself in the transformer is this: If you have a simple harmonic electromotive force the current is distorted; it has complex harmonics; now if you take a condenser, and impress a simple harmonic electromotive force upon it the current will be complex harmonic. But mind you, the difference of potential in the condenser will be a simple harmonic just the same as the secondary electromotive force in the transformer is simple harmonic of the impressed E. M. F. You can plot the curve of hysteresis and other losses, whatever they may be in exactly the same way as you do in the case of iron. You take, not the magnetomotive force but the electromotive force for abscissæ (that is the difference of potential of condenser). For ordinates you plot, not the quantity of magnetic induction, but the quantity of electric induction Q, that is the quantity of electricity, and then you get the points of the hysteresis loop which looks something like curve of Fig. 6. You get the hysteresis loop for condensers just the same as for iron and you can estimate your losses and study how the whole thing acts. You do not only get the losses but you actually get the curve and see how those losses are brought about. Now this is a very interesting thing from a purely scientific standpoint and very important from the engineering standpoint and ought to be studied. Now in the ordinary sliding contact method of plotting curves we cannot determine the quantity of electricity directly; but if we use Mr. Townsend's method we can get that just as accurately as we can determine the voltage by a voltmeter. That seems to me to be a very important point.

MR. TOWNSEND:—With regard to the error of the time constant, it depends, of course, on the ratio of self-induction and resistance in the circuit containing the contact-maker and the instrument. If the resistance is sufficiently high for the current

to have the true form of the rate of change of the E. M. F., it is found in practice that no error due to the time constant of the circuit can be detected.

In order to prove this, I determined a curve with no external resistance in the circuit. In other words, R_2 in Fig. 1 was made equal to zero, the instrument in series with the contact-maker being connected directly across the secondary of the transformer e. Under these circumstances, at a frequency of 120, the error due to the time constant amounted to about two per cent. In this case, however, the method was not correctly used. To comply with the theory, R_2 must be sufficiently great to make the current through the instrument proportional to the time rate of change of the electromotive force to be measured. When this condition is fulfilled, it will be found that the error due to the time constant cannot be detected.

MR. STEINMETZ:—This method of getting the dielectric hysteresis loop of the condenser was first applied, as far as I remember, some years ago by Dr. Bedell at Cornell University. Unfortunately Dr. Bedell has not been so successful as Dr. Pupin in getting a condenser with considerable internal losses but had condensers of very high efficiency. The hysteresis loop he got was so extremely flat and narrow as to be within the errors of observation, amounting to something less than three or four per cent. of the total volt-ampere capacity. But there are condensers which have an excessive internal loss so as to show a large loop and make this phenomenon more pronounced. However, no change of capacity with the voltage was observed by Dr. Bedell nor by me in condensers.

I do not want to be misunderstood by Dr. Pupin. I did not say that I do not care for the magnetic induction, but I did not consider it so very important to get a new method for it, because it can be derived from the current and electromotive force by integration. As a rule, in the usual method of getting instantaneous values of electromotive force you get them say from five to five degrees apart, and then you merely add the total value of 180 degrees and from this value you always add one value at one side,—say for 185°, 190°, etc., and subtract one value at the other side, say 5°, 10° etc., and you have the curve of induction. Now, that can de done for the whole curve in a very few minutes. However, obviously it is an advantage to derive it directly, only it is not so absolutely important.

I fully realize the importance of getting the curve of magnetic induction, the more as for many years I have been satisfied that the general custom of claiming some great and unknown advantages for a sine wave of electromotive force, is not warranted, but that in many cases the sine wave of electromotive force is about the worst you can get. Certain forms of distorted waves give a considerably lower magnetic flux in alternators, transformers and other apparatus, at the same induced E. M. F., and consequently a

higher efficiency. Now you see that it is quite interesting to get the easy way of illustrating why it is, that for instance with the distorted wave of the ironclad unitooth alternator at the same impressed E. M. F. and same frequency, the hysteresis loss in transformers is from eight to twelve per cent. lower than with the sine wave, while certain forms of distributed armature windings give hysteresis losses in excess to those given by a sine wave.

DR. PUPIN:—I am aware, of course, of Dr. Bedell's article, but at that time when the article was published we had already done our work at Columbia University on that very thing, so we still claim the priority although we did not publish anything on the subject.

Another thing, Dr. Bedell's curve of hysteresis was, as Mr. Steinmetz remarked, quite within the errors of observation, so we feel rather uncertain as to whether what he got was really a hysteresis curve or simply a record of his errors. These errors can be obviated by employing very large condenser capacity. We have condensers at Columbia University which mount up to over 240 microfarads, so that we can draw on that as much as we please, and in that way of course we can increase the hysteresis loop to anything we desire. Another point was touched by Mr. Steinmetz, namely, the disputed question whether a peaked electromotive force curve, since it produced a flat induction curve, is not preferable in certain cases, can be decided by experiment in following the use of this contact-maker. It is a disputed question. Mr. Steinmetz maintains (and he does it of course partly for selfish reasons, because he must support his rule of hysteresis loss), that if a curve is a flat curve, although, of course, the area enclosed would be a larger one, the total loss would be smaller, because the loss depends only on the maximum of induction. Now, if that is really so it is a very interesting fact. I am inclined to believe it to be so. But still we would like to have some direct experimental evidence, and if this new method of plotting the curves of induction directly can help us to that, why that of itself is worth all the labors that Mr. Townsend spent on the designing and construction of this sliding contact.

MR. STEINMETZ:—I may say in this respect that not everything is published immediately after it is found out, but sometimes very many years after, even more than seven years. The first proof that the distorted wave gave the lesser hysteresis in the transformer was found by the wattmeter about ten years ago, before the laws of hysteresis were discovered. At the time it was not understood. Afterwards I hunted up the old tests and found the first experiment regarding the effect of the wave distortion or rather the effect of different types of alternators made by wattmeter reading, dating back before the time of my studies of the hysteresis law.

DR. PUPIN:—Yes, but I am afraid if we go into this discussion there will not be any time left for this paper, because I do not

believe implicitly in the wattmeter readings. I would not accept them without an investigation as to the construction of the wattmeter and the way that it was used; so that I would not pay much attention to that. We all know that wattmeter readings are not reliable when we have harmonics in the circuit; that special precautions must be taken to ensure the accuracy of the readings. Now whether in these wattmeter readings to which Mr. Steinmetz referred, those precautions were taken or not I do not know. I do not think they were.

MR. STEINMETZ:—I do not quite agree that wattmeter readings are not reliable. It is true that theoretically an error is introduced by the inductivity of the potential circuit, and in scientific investigations of extreme accuracy this error would undoubtedly be serious. In practice, however, where as a rule no attempt is made to get a higher accuracy than one-fourth per cent., this wattmeter error is generally negligible.

Sometime ago I made an investigation in this respect and found that with certain commercial voltmeters the inductance error is still below one per cent. at 1500 cycles. The wattmeter error is of the same magnitude for non-inductive load and increases with the increasing phase displacement of the main current, but becomes noticeable only at very low power factors, below 20%. The exciting current or open circuit current of a transformer, however, has quite a high power factor, from 50 to 65%, and in this case the wattmeter error due to the fundamental wave, is still negligible, that due to the higher harmonic small, and since the power of the higher harmonics is a small part of the total power, the maximum possible error due to the wattmeter is of far lower magnitude than the difference of hysteresis loss observed with different wave shapes.

There being no further discussion, the following paper entitled "Notes on Single-Phase Induction Motors and the Self-Starting Condensing Motor," was read by Mr. Steinmetz. [See January issue p. 37.]

DISCUSSION:

DR. PUPIN:—I would like to ask Mr. Steinmetz to make a picture of how the armature stood and the secondary circuit and the tertiary circuit and the condenser. I cannot make out from the paper what the exact arrangement was.

MR. STEINMETZ:—The primary or impressed circuit is mounted on the stationary member of the motor. The secondary circuits are distributed over the rotating member in polyphase winding. The tertiary circuit is arranged on the stationary member of the motor, thus in constant relative position to the primary circuit, theoretically at 90° angular displacement. Applied to the three-phase wound motor with star or ring connected stationary winding, of terminals, 1, 2, 3, terminals 1 and 2 are connected to the primary impressed E. M. F., terminals 1 and 3 to the condenser as tertiary circuit, and referring to the last chapter of the paper, the inductance used as second tertiary circuit is connected to terminals 3 and 1.

DR. PUPIN:—It seems to me that there is a reaction there between the circuits. Take the first case where Mr. Steinmetz put the two circuits first at right angles to each other and puts a condenser in one of the circuits. Well, the two will still react upon each other through the secondary. Now it does not make any difference whether that condenser circuit reacts upon the primary directly or indirectly, the reaction will be there. It is as far as I can see through it—of course I do not pretend to be able to analyze completely the whole arrangement at a moment's notice,—I have not seen the paper before—but I think it is just the same case as that investigated by myself in 1895, the effect of a secondary circuit on a primary when the secondary contains a condenser in it, which I at that time called *consonance*, and which was investigated by Mr. Steinmetz afterwards, and by Feldman in Germany. It is simply this: You can influence the phase of a primary circuit by a condenser in the circuit itself, which is not always an advisable thing to do because that circuit may have a very large self-induction and very high voltage, and if you put a condenser in there, either in series with that self-induction or in parallel, you will have trouble. You will have very large rise of potential and probably a breaking down of the condenser. So at that time I suggested that it would be advisable to use a secondary circuit with a small self-induction, and large capacity. You could get that way a large current, therefore a large magnetomotive reaction, and therefore a powerful regulation of the phase of the primary current. So that looking upon those matters in this way, I should be inclined, I suppose, partly for selfish reasons, to think that Mr. Steinmetz's arrangement is a particular case of electric consonance. I do not mean to claim an anticipation of the ideas contained in this paper. But I mean to say that, analyzing Mr. Steinmetz's arrangements to

its last scientific principles, I would say they are about the same thing as electrical consonance. I do not, of course, wish to say anything that might in the least diminish the credit for the work which Mr. Steinmetz describes in his able paper. In dealings with circuits like that, if you employ condensers you feel just like a baby walking on a slippery floor. The work is exceedingly delicate and laborious and sometimes interesting, sometimes very tedious. It requires a large amount of work before you get things right. It seems to me that Mr. Steinmetz has experienced that, because in his paper he seems to use coaxing terms. Where we say "reactance" of the circuit and "impedance" of the circuit, he uses coaxing terms as "admittance" and "permittance," as if to say: Just come in, dear current, don't be afraid, and he seems to get the current to come in and do the work he wanted it to do.

MR. CHAS. S. BRADLEY:—In 1895 I did considerable work on this line of motors and it was partially suggested by Dr. Pupin's work on consonance. There are several ways of arranging these circuits and I have always believed that one of the best was to have one circuit at one angle and another one at a slight angle say, at an angle of about 60 degrees, with the inductance in it. Imagine that this is the secondary (referring to sketch) wound for three-phase, with a three-phase condenser, a slight effort of rotation produced by an angular coil will cause a three-phase current to be set up in the secondary and a rotation of the magnetism, while the armature is standing still, and with the three-phase condenser the conditions are more thoroughly established so that it gives the motor a directive effort at once if the inductance is properly proportioned to the condenser, the condenser will then take off all the lag due to the additional inductance and also the inductance of the motor. The motor is self-starting and is a consonance motor, its secondary circuit being in a consonance relation with the primary and by increasing and decreasing its capacity the motor can be made to give full torque from start to complete load.

MR. STEINMETZ:—There is one essential difference between the arrangement discussed by me and the application of the condenser in a transformer secondary called by Dr. Pupin, "consonance," or Mr. Bradley's condenser motor, and that difference is that the condenser in my arrangement is not in a secondary circuit to the primary impressed circuit.

Thus, taking the theoretically simplest case of a tertiary circuit in stationary quadrature position to the primary circuit, with a single-phase E. M. F. impressed upon the primary circuit and the motor at standstill, a voltmeter in the condenser circuit shows no voltage at all, that is the condenser is neither in consonance relation nor in any other way induced by the primary circuit. Only when the motor begins to rotate, voltage and current appear in the condenser circuit, increasing with the speed, and induced by

the secondary currents flowing in the short-circuited revolving armature, which as I have explained, are carried by the rotation more or less into quadrature position to the impressed circuit, and thus into inductive relation to the tertiary or condenser circuit.

This difference is very important and essential, since it makes the operation of the condenser independent of the wave shape of the primary E. M. F. Whatever may be the wave shape of primary impressed E. M. F., the magnetic flux interlinked with the secondary circuit is absolutely constant and uniformly rotating, since it passes the short-circuited low resistance secondary winding, and since only the secondary magnetic flux can reach and induce the condenser circuit, the E. M. F. induced in the latter is a perfect sine wave irrespective of the wave of primary impressed E. M. F.

The term "admittance" is nothing new, but was introduced by me years ago in denoting the reciprocal of "impedance."

With regard to the liability of excessive rise of voltage and the need of carefully adjusting the condenser capacity, this does not exist with the condenser in the tertiary circuit of the induction motor.

While the general rule is to use a volt-ampere capacity of condenser equal to the volt-ampere input of the motor running light, no need exists of being so particular about the amount of condenser, but no harm results if you put 50% more or less condenser in circuit. With the right amount of condenser we get 100% power factor. By using 50% more we perhaps get 95% power factor, somewhat more starting torque, and a slightly leading current, and with 50% less condenser perhaps 95% power factor, somewhat less starting torque and slightly lagging current.

Very careful adjustment and excessive rise of voltage are features incident to the use of capacity in series to inductance. But there is an essential difference between capacity in series and capacity in shunt, as pointed out some years ago by Mr. W. Stanley and me.

Capacity produces leading currents. Thus, if you connect a capacity in shunt to a circuit with lagging current, as an induction motor, whether directly or through transformer, the lagging current is reduced by as many volt-amperes as the condenser gives leading current, but no rise of voltage occurs.

If the volt-amperes condenser are equal to the lagging volt-amperes of the circuit, the latter will be entirely compensated and disappear; if less condenser is used, some lagging current remains; if more condenser, some leading current remains after neutralizing the lagging current.

This feature I use in the induction motor. The lagging component of induction motor current is neutralized by the leading current of the condenser in the tertiary circuit, and thus disappears more or less completely. As easily seen, no great accuracy of adjustment is needed, but 20% condenser more or less does not change the power-factor noticeably from unity.

Different it is with the use of the condenser in series to an inductance. By inserting a capacity reactance equal to induc-

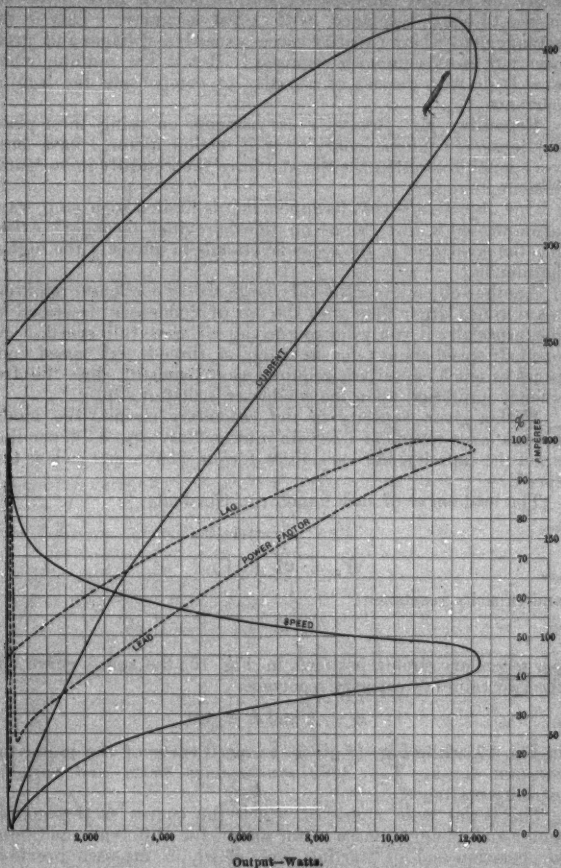

FIG. 16. Induction Motor. Load curves. Capacity Reactance in Secondary.

$$k_1 = .2$$
$$Z = .1 - .8j \quad Y = .01 + .1j.$$

tive reactance in series, the E. M. F. of self-induction is balanced against that of the capacity and the total impressed E. M. F. re-

mains available for sending current through the resistance of the circuit. Thus the current rises excessively, and correspondingly the voltage, but capacity and inductance have to be adjusted to equal reactance, with great accuracy. This is the arrangement where capacity causes excessive voltages and great accuracy of adjustment is required. But it has nothing whatever to do with the use of capacity to compensate for lagging currents.

Regarding Mr. Bradley's experiments on using three-phase condensers in the secondary of an induction motor, I have been very much interested therein and have theoretically investigated it some time ago.

One objection is that its application is limited to a sine wave of impressed E. M. F., since no short-circuited secondary exists, but the condenser in the secondary tends to intensify the higher harmonics.

Another objection is the very large size of condenser required, many times larger than with the use of condenser described by me, especially if you want to run up to near full speed. At one-half speed about twice as much, at three-fourth speed four times as much capacity is required as at standstill, since the frequency impressed upon the condenser is that of slip.

With a given capacity in the secondary circuit, the motor gives a large torque at a certain intermediary speed, depending upon the capacity, but very little torque above and below this speed.

The load curve of such a motor of constants:

$$Z_0 = Z_1 = .1\text{–}.3\,j$$

$$Y_0 = .01 + .1\,j,$$

and capacity reactance:

$$k = .2$$

per secondary circuit, is shown in Fig. 16; the speed curve for the two values of capacity reactance:

$$k = .2 \text{ and } k = 1.0$$

in Fig. 17, while Fig. 18 gives the speed curve of the motor for constant torque $T = 4800$ synchronous watts, and variable capacity reactance.

This is the same motor of which the curves as three-phase motor and as single phase motor under the different conditions of operation and of starting, are shown in my two previous papers of 1897 and 1898.

MR. BRADLEY:—I have a large volume of actual work, observations from motors of this kind, and with the permission of the INSTITUTE I will read a paper on this subject at a future time.

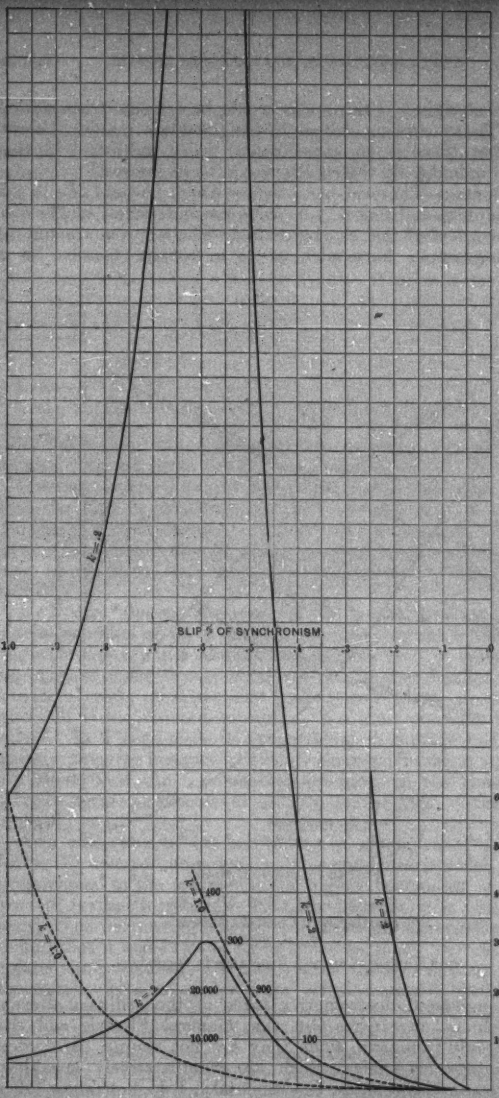

FIG. 17. Induction Motor. Speed Curves. Capacity Reactance in Secondary.

$k = 1.0$ and $k = .2$.

$Z = .1 - .3j \quad Y = .01 + 1j$.

MR. STEINMETZ:—I think that would be extremely interesting for the TRANSACTIONS. I have investigated this matter theoretically, but there is no experimental work published in that line and undoubtedly the results would be of interest.

DR. PUPIN:—There is one question which I would like to ask Mr. Steinmetz, and that is, I do not quite understand why he considers the effect of that short-circuited secondary on account of its killing upper harmonics as so important, because as a

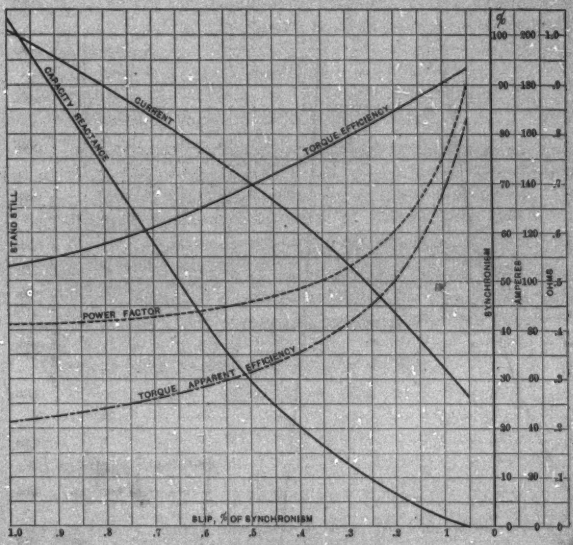

FIG. 18. Induction Motor.

$Z = .1 - .8j \quad Y = .01 + .1j.$

Starting with Constant Torque

$T = 4800$

And Variable Capacity Reactance in Secondary.

rule the impressed electromotive force of the ordinary alternators, such as are made by the General Electric and the Westinghouse Company, when they are not simple harmonics, have complex harmonics which are comparatively speaking, weak in comparision to the fundamental. I do not know of a case that is a very serious exception to that rule. Well, the

fundamental will do the principal work if you allow it to do so. Now if you use a tertiary circuit, or a secondary circuit for that matter, with a condenser in it and you give that condenser the proper capacity to assist the fundamental of the primary, that is all which you need do. Who cares what the other harmonics will do? Because they are not so very important anyhow, and you are simply working as if the upper harmonics were not present at all. Everybody knows that you can treat all these independent components entirely independently; treat each one by itself as if the others were not present. So that even if the upper harmonics are there, in the tertiary circuit, provided the capacity is such as to assist the fundamental and not assist the upper harmonics, I think the circuit will work just as well. The capacity introduced there will not help the upper harmonics to develop an undesirable current there. It will simply assist the fundamental. Of course, I would like Mr. Steinmetz to point out more particularly the importance of watching the upper harmonics, I do not really see the importance of it.

MR. STEINMETZ:—It is true the motor will run just as well with the higher harmonics present. It will run also without condenser if once started. But without condenser it runs with poor power factor due to excessive lagging currents, and the main object of the condenser is to compensate for these lagging currents.

As I have shown, however, in the paper, it does not take much distortion of wave shape to produce so much current of higher frequency in a shunted condenser, that in spite of the elimination of the fundamental wave of lagging current the total current actually increases, that is the power factor drops by the insertion of the shunted capacity. We must consider that the current in a condenser is proportional to voltage and to frequency, thus for instance a seventh harmonic in the E. M. F. wave of only 10% of the fundamental, produces in the condenser a seventh harmonic of current of 7×10. or 70% of the fundamental.

As a rule, due to the self-inductive reactance of the transformer or compensator feeding the condenser, which is also proportional to the frequency, the higher harmonics of current are still further increased. Thus, for instance, it is nothing unusual when connecting a condenser across an alternating circuit to find that it takes two or three times as much current as calculated from capacity, frequency and voltage, and to measure the capacity of a condenser from volt and ampere readings, it is necessary to insert in series to the condenser a very high resistance or inductance to eliminate the effect of the higher harmonics.

I am somewhat inclined to suspect that the distortion of the current wave which Dr. Pupin observed in his condenser tests and attributed to a kind of dielectric hysteresis, was nothing but the higher harmonics of current produced by the higher harmonics

of the impressed E. M. F., the latter being too small to be directly observed, while when multiplied in the condenser they were noticed.

DR. PUPIN:—That is the first thing a man ought to think of when he gets distorted current, whether that is not due to the presence of upper harmonics in the impressed electromotive force. To overlook that, of course, would be a very serious error on the part of a man who knows anything about experimental work. In the first place I analyzed that alternator a year and a half or two years before that, in 1894, and I knew exactly that the fundamental was very, very much larger than the upper harmonics. In fact of the upper harmonics there was only one that amounted to anything at all and that was the third harmonic which was a very small percentage of the fundamental. I have forgotten now, but it was something like one or two per cent. Now if this current were due to the presence of the upper harmonics, the shape of the current curve would not depend upon the voltage at all. It would remain just the same no matter whether you have a small voltage or a large voltage. Well now, as a matter of fact, the shape of the current depended very much on the voltage. It shows that as we increase the voltage, of course, the leakage and the specific inductive capacity will increase, and, of course, the deviation of the current from the simple harmonic form will increase much.

The next point is this: Mr. Steinmetz says that if you introduce a condenser in the presence of the upper harmonics you get very large condenser currents, that is so large that the condenser current in the fifth harmonic is just as large as that of the fundamental. That I do not admit for one moment. I think if you arrange your condenser properly, say that the fundamental of the impressed electromotive force is six times as large as the fifth harmonic if you would arrange your condenser properly, the fundamental current ought to be not only six times as large but at least thirty times as large as the fifth harmonic—at least that. The arrangement is ineffective if you do not get as much effect as that.

MR. BRADLEY:—It all depends upon the adjustment of the condenser and inductance. If they are adjusted for fundamental, even the third harmonic is choked-out, especially if it is in the secondary. In order for these motors to work well, the adjustment should be very nicely made; that is the condenser and inductance should be adjustable for what Dr. Pupin calls consonance if it is in the secondary; if it is in the primary it ought to be for resonance. If it is in resonance for the fundamental the current is almost entirely chocked out for the third or fifth or any other harmonic. Only the fundamental is augmented. The others are dampened.

MR. STEINMETZ:—In the last discussion Dr. Pupin and Mr. Bradley have been speaking of something entirely different

from what I am using. I referred to capacity in shunt to the lagging circuit, while their remarks apply only to capacity in series to inductance with its inherent defects of excessive voltage rise and excessive care of adjustment.

Series capacity was tried by Mr. Wm. Stanley some years ago, but given up as commercially impracticable, since the unavoidable variations of frequency in commercial circuits are so large as to throw the adjustment between capacity and inductance out of balance. Thus any attempt at using series capacity must be doomed to failure, except when used as by Mr. Bradley in a movable secondary circuit, which adjusts its frequency to that of balance between capacity and inductance.

With shunted capacity as the only way in which capacity can be used practically, there is no adjustment for this or that harmonic, but each harmonic takes current in proportion to its E. M. F. and frequency.

Regarding Dr. Pupin's remark that the generator for such induction motor should not give any higher harmonics, we must consider that in single phase circuits, mostly small and medium size systems of 125 cycles, the motor load is an incidental feature only, which while quite desirable is not of sufficient importance to warrant the installation of a type of generator which increases the continuous loss of energy in the transformer cores by something like 10%. Still less can it be expected to throw out the existing generators and install new ones to accommodate a few induction motors. Thus, the problem of the single-phase induction motor is to build them so as to run satisfactorily, with higher power factor, and fair efficiency, on such E. M. F. waves as given by the existing single-phase systems.

Regarding Dr. Pupin's observation that the shape of the current wave impressed upon a condenser changes with the impressed voltage, this rather tends to point to my explanation, of these higher harmonics of current being due to higher harmonics of the impressed E. M. F, since when changing the voltage by changing the excitation of an alternating current generator, usually the shape of the E. M. F. wave, that is the relative values of its harmonics vary more or less.

DR. PUPIN:—These harmonics were studied with very great accuracy. I used the resonance methods of analysis. We could detect as much as the fifteenth harmonic by the use of the telephone. But with an accurate voltmeter we could not detect anything beyond the third, all the others were too small. About that adjustment for the fundamental and the upper harmonics, Mr. Steinmetz says that if you use a condenser in the secondary circuit that you do not get these rises of current so much, that simply all you have to do is to compensate for phase. To be sure that is very true. You do not get a rise in the primary current. The primary current is only affected

with regard to its phase. But the circuit which contains the condenser, that current increases very much, and if you adjust your capacity there so as to produce a very large effect upon the phase of the primary current, then the current in the tertiary or secondary circuit containing the condenser will be very large. That is my point. It must be so. How otherwise could a few turns in the secondary circuit with a condenser in it produce a large effect on the phase of the primary unless you had a very large current in that secondary. That phase effect is only produced by the counter-magneto force being produced there. If you want to produce there a sufficiently strong counter-magneto force with a few turns you must necessarily have a large current.

MR. STEINMETZ:—The object of the condenser in the induction motor is to produce a minimum current input, that is approximately 100% power factor, by compensating for the wattless magnetizing current of the motor. In this case the amount of capacity required is moderate and the current in the condenser circuit is correspondingly small. There is indeed a second value of capacity giving 100% power factor with maximum current input, in balancing the internal self-induction of the motor.

In this case an excessive rise of voltage occurs. This case takes some 10 to 20 times as much capacity as used, and causes currents to flow far beyond the heating limit of the motor, and thus is of no practical importance.

DR. PUPIN:—I have investigated that point. I call that the second critical point. That is not the point at which you work. You work at a point where you know the current will remain small in the primary circuit.

MR. STEINMETZ:—The actual condition of the motor corresponds to what Dr. Pupin calls the "first point of consonance," but as I pointed out when Dr. Pupin published his investigation some years ago, the introduction of the term consonance for both critical points is very unfortunate, since it combines entirely different phenomena in one name and thus leads to misunderstanding and confusion, as to-day's discussion has shown again.

[Adjourned.]

[Communicated after Adjournment by W. S. Franklin.]

I have been much interested in the discussion of wireless telegraphy published in the December number of the Transactions. Dr. Pupin, as it seems to me has made several erroneous statements in calling attention to the great importance of a small logarithmic decrement in the transmitting and receiving circuits. In the first place he would have it appear that the logarithmic decrement depends mainly upon the resistance of the respective circuits, while it is well established theoretically that the dissipation of energy by radiation is much more prominent in determining the decrement of electrical oscillations except in those types of oscillators which do not radiate to any great extent, which types are not suitable for transmitting circuits. In the second place the use of successive impulses in the sending circuit for increasing the oscillation in the receiving circuit does, as it seems to me, require each successive impulse to be in proper *step* with the foregoing impulse as pointed out by Mr. Mailloux. To realize this condition in practice would be next to impossible, and it seems to me that we must be content with the oscillations produced in the receiving circuit by one impulse in the sending circuit with its trail of decaying oscillations.

In the third place, the electrical oscillations of a conducting staff perpendicular to an infinite conducting plane, are precisely the same as the oscillations of an isolated staff of double length, (the resistance of the plane being negligible) Dr. Pupin would have it appear that the staff and plane is a distinct and much more complicated oscillating system.

In the fourth place it does not seem that small logarithmic decrement is of any importance in determining the range of signaling, for the decrement is due mainly to radiation of energy and the intensity of the oscillations produced in the receiving circuit by a few intense waves is about the same as would be produced by a greater number of less intense waves representing the same total amount of energy, although the receiving circuit would need to be more accurately tuned in the latter case to give the full effect and therefore, the latter case makes it more feasible to realize selective signaling. Also slow oscillations (and long wave-length) allow the receiving wire to gather into itself a wider band of the passing waves, so that a greater effect would be produced by a given amount of energy expended in the sending circuit.

[COMMUNICATED BY M. I. PUPIN IN REPLY TO PROF. FRANKLIN.]

[See page 651.]

Professor Franklin quotes four erroneous statements in my discussion. I shall consider these in order of importance, beginning with the least important. "The electrical oscillations," says Professor Franklin, "of a conducting staff perpendicular to an infinite plane are precisely the same as the oscillations of an isolated staff of double length. Dr. Pupin would have it appear that the staff and plane is a distinct and much more complicated oscillating system."

This is a very sweeping statement and should be backed up by a reference to the authorities on which it rests. Besides the oscillatory system which I discussed is not correctly stated by the Professor. It is an infinite horizontal conducting plane with a vertical staff forming a part of it, *then an air-gap*, and then another vertical staff. The air-gap is the center of disturbance. If the learned Professor means that the oscillations on such a system are the same as on an isolated wire consisting of the vertical staffs above the horizontal plane plus two other vertical staffs below the horizontal plane of the same length as the two staffs above the plane, then, in my humble opinion, he is wrong. If he does not mean that, then I fail to see the point of his criticism.

"In the second place," says Professor Franklin, "the use of successive impulses in the sending circuit for increasing the oscillation in receiving circuit does, as it seems to me, require each successive impulse to be in proper step with the foregoing impulse as pointed out by Mr. Mailloux. To realize this condition in practice would be next to impossible and it means to me that we must * * * ."

It is evident that the resultant of two waves is greater than any one of the two components unless the difference in phase between them is considerably greater than a quarter of one period. Now a simple consideration will show that if we communicate to a conductor several series of waves, these series succeeding each other at equal intervals of time, that the waves thus set up in the conductor will continually approach the incoming waves in phase. In fact the phase equality will be reached very rapidly. But in order that the waves already set up in the receiving conductor should be aiding the incoming waves in phase equality is not at all necessary. All that is required is a phase difference smaller than a quarter period. This tendency of the waves set up in the receiving wire to get into step with the incoming waves may be called the accommodation of phase and may be easily demonstrated by a few simple force parallelograms.

"In the fourth place," to quote again Professor Franklin, "it does not seem that small logarithmic decrement is of any importance in determining the range of signaling. * * * ."

Well, I am not aware that I ever said that it was. Why then, accuse me of an offence of which I am not guilty. All the virtues I claimed for waves of small decrement is a power of producing strong resonance and, therefore, selectivity.

I come now to the last offence which is charged against me. "In the first place," says Professor Franklin, "he would have it appear that the logarithmic decrement depends mainly upon the resistance of the respective circuits, while it is well established theoretically that the dissipation of energy by radiation is much more prominent in determining the decrement of electrical oscillations except in those types of oscillators which do not radiate to any great extent. * * * "

Unless I am much mistaken, Professor Franklin is evidently mixing up things which do not belong together. That which is well established theoretically is simply this: If electrical oscillations are set up in a particular way in certain conductors like a sphere or a short cylinder then the damping due to radiation is very much greater than that due to the heat losses produced by the ohmic resistance of the material of which the oscillators are made. The two particular ways of excitation which the theory succeeded in solving are these: (*a*) Sudden destruction of an electric field, (*b*) sudden destruction of a magnetic field in which the oscillators are placed. These two ways of starting electrical oscillations are entirely different from the way in which electrical oscillations are set up in the case before us. Here a higher difference of potential is established between two parts of the oscillator, the two parts being separated by an air-space. A breaking down of the air-gap starts the oscillations and the air-gap itself becomes a part of the oscillator. We have here a heterogeneous oscillator, one part of which possesses a very high resistance. Mathematical theory has not succeeded yet in deciding how much of the damping in a case of this kind is due to radiation and how much to ohmic resistance. It should be observed now that this matter will depend much on the frequency of oscillation. The higher the frequency the more prominent becomes the damping due to radiation I have not the slightest doubt that with low frequencies such as are employed in wireless telegraphy and where an air-gap of a good fraction of an inch is employed, most of the damping is due to the resistance of the air-gap. The matter is capable of experimental test. Take two long rods, place them vertically and through the air-gap between them pass sparks. At a distance take two equal rods and make the air-gap between them minute. See now whether you get better inductive effects with a large or with a small primary gap.

According to Professor Franklin, the inductive effects should be proportional to the square of the spark length. That such is not the case is shown by Professor Fessenden. If the receiving rods are carefully tuned it would be found that the

inductive effects, even with short rods, say twenty feet, where the frequency is very much higher than the one employed in wireless telegraphy are often even better with short sparks than with long ones. This would certainly be impossible if the damping is due principally to radiation. Why, Marconi himself prefers sparks of quite moderate length to long ones. Now if the damping is due principally to radiation why not employ long sparks?

I will state here, however, that in an arrangement like the two vertical wires used for transmitting in wireless telegraphy the principal part of the energy radiated off leaves, in all probability, the transmitting wire during the first half-oscillation immediately following the spark, and the rate of radiation during this interval is entirely different from the rate which attains after the first half-oscillation. I came to this conclusion sometime ago, after the date at which this subject was discussed before the INSTITUTE. I do not think that it will be well to discuss this matter at present. Suffice it to state that this damping due to this initial radiation is an entirely different story from the damping which accompanies the stationary state of oscillations and which Professor Franklin has in mind.

American Institute of Electrical Engineers,

COMING MEETINGS.

NEW YORK, April 7th, 1900.

The Annual Meeting of the Institute for the election of officers, receipt of annual reports, etc., will be held on Tuesday evening, May 15th, 1900, at 12 West 31st Street, New York.

The General Meeting for the reading and discussion of papers will begin at Philadelphia on the following afternoon, Wednesday, May 16th, and continue probably on the 17th and 18th.

THE PARIS MEETING,

THURSDAY, AUGUST 16th, 1900.

The proposed programme for the Paris meeting in August next will embrace various attractive social features. Mr. T. C. Martin, of the Committee, cables from London that the Institution of Electrical Engineers will offer a dinner in London to the visiting members of the INSTITUTE on August 13th. The members of both bodies will take a special train to Paris on August 15th. The joint meeting will be held in the United States building at the Paris Exhibition on August 16th and on the evening of the same day a reception will be held at the British building, the headquarters of the British Royal Commission. About 70 members have reported that they expect to attend the Paris meeting.

Those members who expect to sail early in the season may be interested in the following invitation which has been extended to American Engineers.

THE INSTITUTION OF CIVIL ENGINEERS,
Great George St., Westminster, London.

January 26th, 1900.

RALPH W. POPE, ESQ., Secretary,
AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS,
26 Cortlandt Street, New York, N. Y.

DEAR SIR:

It is my pleasure to acknowledge the receipt of your letter of the 16th instant, and to say that Sir Douglas Fox (the President) is very glad, indeed, to learn that he may possibly have the pleasure of welcoming a number of your members in London on their way to Paris.

May I take this opportunity of saying that it has just been arranged to hold the Annual Conversazione of the Institution on Thursday, the 5th July, when the President and Council hope that the large number of members of this Institution from all parts of the country, who are drawn annually to that function, may have an opportunity of meeting many of their American professional brethren. A formal invitation in respect of your visit to this country will presently be sent, when its date is more fully determined on by you; but I meanwhile mention this fixture, as it has just been made.

Hoping to hear further from you after the Council meeting to which you refer,

Believe me, yours faithfully

(Signed) J. H. T. TUDSBERY, Secretary.

All of the National Engineering Societies have found it impracticable to arrange for special transportation on account of the various sailing dates selected by their members. Members of the Institute will be expected, therefore, to make their own arrangements.

Report of Committee on the Standardization

OF

ELECTRIC GENERATORS, MOTORS AND TRANSFORMERS.

This Report which has been in preparation since January 1898, has now been published for general distribution. It is receiving the endorsement of leading manufacturers and engineers.

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COPPER WIRE TABLE.

This table gives the WEIGHTS, LENGTHS and RESISTANCES of cool, warm, and hot wires of

MATTHIESSEN'S STANDARD CONDUCTIVITY,

for both Brown and Sharpe (A. W. G.) and Birmingham Wire Gauges (B. W. G.)

It was prepared by the Institute Committee on Units and Standards, personally revised by the individual members of the Committee, and issued with the October, 1893, number of the TRANSACTIONS, forming a part of Volume X.

Mailed separately, price 10 cents.

RALPH W. POPE, Secretary,
26 Cortlandt Street,
New York City.

Contents of Vol. XV, 1898.

THE DEVELOPMENT OF ELECTRIC CABLEWAYS. (Illustrated.) By Richard Lamb. SPARKING, ITS CAUSE AND EFFECTS. (Illustrated.) By Thorburn Reid. WIRELESS TELEGRAPHY. By W. J. CLARKE. No 1, January, 1898. THE STANDARDIZING OF GENERATORS MOTORS AND TRANSFORMERS. (A Topical Discussion.) SINGLE PHASE INDUCTION MOTOR. (Illustrated.) By Charles P. Steinmetz. No. 2, February, 1898. INSULATION AND CONDUCTION. (Illustrated.) By Reginald A. Fessenden. No. 3, March 1898. AN ECONOMY TEST OF A CENTRAL STATION. (Illustrated.) By W. E. Goldsborough. A NOVEL FORM OF THERMO-ELECTRIC BATTERY. (Illustrated.) By Charles J. Reed. No. 4, April, 1898. DIELECTRIC STRENGTH OF AIR. (Illustrated.) By Charles P. Steinmetz. TWO-WIRE DISTRIBUTING SYSTEMS AND LAMPS, 200-240 VOLTS. By John W. Howell. A CAPILLARY ELECTROMETER FOR ELECTRICAL MEASUREMENTS. (Illustrated.) By Charles F. Burgess. ALTERNATING CURRENT TRANSFORMERS FROM THE STATION MANAGER'S VIEWPOINT. By W. F. White. SOME PHASES OF THE RAPID TRANSIT PROBLEM. (Illustrated.) By A. H. Armstrong. PRELIMINARY REPORT OF THE COMMITTEE ON STANDARDIZATION. POWER TRANSMISSION AND DISTRIBUTION FOR RAILWAY WORK. (Illustrated.) By Ernest J. Berg. THE COMMUTATED CURVE OF A COMPOSITE-WOUND ALTERNATOR. (Illustrated.) By D. C. Jackson. SOME TESTS WITH AN INDUCTION GENERATOR. (Illustrated.) By A. F. McKissick. THE EVOLUTION OF THE LINE SIGNAL. (Illustrated.) By Arthur V. Abbott. A MODERN ELECTRIC CENTRAL STATION. (Illustrated.) By George A. Damon. THE GRAPHICAL TREATMENT OF ALTERNATING CURRENTS IN BRANCHING CIRCUITS. (Illustrated.) By Dr. H. T. Eddy. No. 5, May, 1898. THE PRESENT STATUS OF ELECTRICAL ENGINEERING. (Inaugural Address.) By A. E. Kennelly, President. No. 6 and 7, June and July, 1898. THE PHOTOMETRY OF THE ENCLOSED ALTERNATING ARC. (Illustrated.) By C. P. Matthews, W. H. Thompson, and J. E. Hilbish. SOME TELEPHONE DISTURBANCES FROM ELECTRICAL GENERATORS. (Illustrated.) By George D. Shepardson. AIR GAP AND CORE DISTRIBUTION. (Illustrated.) By W. E. Goldsborough. No. 8 and 9, August and September, 1898. AN ELECTRICAL SURVEY IN THE BOUOUGH OF MANHATTAN. (Illustrated.) By A. A. Knudson. HIGH VOLTAGE POWER TRANSMISSION. (Illustrated.) By Charles F. Scott. No. 10, October, 1898. THE DESIGN OF TRANSFORMERS. (Illustrated.) By Frederick W. Carter. No. 11, November, 1898. CATALOGUE OF MEMBERSHIP. No. 12, December, 1898.

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THE EDUCATION OF ELECTRICAL APPRENTICES AND JOURNEYMEN. By Arthur A. Hamerschlag. ELECTRICITY ON BOARD SHIP. By S. Dana Greene. No. 1, January, 1899. STORAGE BATTERIES AND RAILWAY POWER STATIONS. (Illustrated.) By R. McA. Lloyd. No. 2, February, 1899. PROPAGATION OF LONG ELECTRICAL WAVES. (Illustrated.) By M. I. Pupin. No. 3, March, 1899. THE LIMITATIONS OF POWER SUBDIVISION BY ELECTRIC MOTORS IN MANUFACTURING ESTABLISHMENTS. A TOPICAL DISCUSSION. No. 4, April, 1899. THE MULTIPLE UNIT SYSTEM OF ELECTRIC RAILWAYS. (Illustrated.) By Frank J. Sprague. No. 5, May, 1899. REPORT OF THE COMMITTEE ON STANDARDIZATION. SYMBOLIC REPRESENTATION OF GENERAL ALTERNATING WAVES AND OF DOUBLE FREQUENCY VECTOR PRODUCTS. (Illustrated.) By C. P. Steinmetz. THE COST OF ELECTRICITY IN SOME TYPICAL BUILDINGS IN NEW YORK CITY. (Illustrated.) By P. R. Moses. Nos. 6 and 7, June and July, 1899. THE DETERMINATION OF THE WAVE FORM OF ALTERNATING CURRENTS WITHOUT A CONTACT MAKER. (Illustrated.) By Harris J. Ryan. ELEMENTS OF DESIGN FAVORABLE TO SPEED REGULATION IN PLANTS DRIVEN BY WATER POWER. (Illustrated.) By Allan V. Garratt. THE PROTECTION OF SECONDARY CIRCUITS FROM FIRE RISKS. By Cary T. Hutchinson. PLEA FOR A WORKING STANDARD OF LIGHT. By S. Ev. rett Doane. MOTOR SPEED REGULATION. By Alton D. Adams. Nos. 8 and 9 August and September, 1899. ELECTRICITY IN COAL MINING. (Illustrated.) By J. F. Jackson and Frank P. Thompson. AIR GAP AND CORE DISTRIBUTED. (Illustrated.) By W. E. Goldsborough. TEST OF A 300-KILOWATT DIRECT CONNECTED RAILWAY UNIT AT DIFFERENT LOADS. (Illustrated.) By E. J. Willis. No. 10, October, 1899. OPERATING COSTS OF HORSE AND ELECTRIC DELIVERY WAGONS IN NEW YORK CITY. (Illustrated.) By Geo. F. Sever and R. A. Fliess. ELECTRIC AUTOMOBILES. (Illustrated.) By Elmer A. Sperry. SERIES ARC LIGHTING FROM CONSTANT CURRENT-TRANSFORMERS. (Illustrated.) By William Lispenard Robb. COST OF ARC LIGHTING. (Illustrated.) By H. H. Wait. AN AMERICAN PACIFIC CABLE. (Illustrated.) By George O. Squier. No. 11, November, 1899. THE POSSIBILITIES OF WIRELESS TELEGRAPHY. (A Topical Discussion.) New York and Chicago, Nov. 22, 1899. No. 12, December, 1899.

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NOTES ON ELECTRIC TRACTION UNDER STEAM RAILWAY CONDITIONS. By Edward C. Boynton. NOTES ON SINGLE-PHASE INDUCTION MOTORS. (Illustrated.) By Charles P. Steinmetz. A NEW METHOD OF TRACING ALTERNATING CURRENT CURVES. (Illustrated.) By Fitzhugh Townsend. No. 1, January, 1900. THE EVOLUTION OF SAFE AND ACCURATE FUSE PROTECTIVE DEVICES. (Illustrated.) By Joseph Sachs. No. 2, February, 1900. HYSTERESIS IN SHEET IRON AND STEEL. (Illustrated.) By Arthur Hillyer Ford. No. 3, March, 1900.

DATE OF ANNUAL AND GENERAL MEETINGS.

At the meeting of the EXECUTIVE COMMITTEE, on February 28th, it was decided that the Annual Business Meeting of the INSTITUTE for the election of officers, and reception of the yearly reports, would be held in New York City, on Tuesday, May 15th, being the third Tuesday in the month, as required by the Constitution.

The General Meeting for the reading and discussion of professional papers will begin at Philadelphia, in the afternoon of the following day, Wednesday, May 16th, and continue for two or three days, as may be determined upon hereafter. The meeting will then adjourn to Paris, August 10th, 1900.